continue to go on thus, the nineteenth century will be positively a blank in the history of architecture, and, unlike that of Elizabeth, the reign of Victoria an absolute nullity. With No. 1201, "New Buildings in the Temple," *S. Smirke*, we are by no means so well satisfied. Not only are they more continental than English in physiognomy, but they do not at all accord with any thing near them; on the contrary, are quite a patch stuck upon that range of buildings which they are intended to carry on and complete. While others are effacing and defacing Soane's works, Mr. Smirke seems to be anxious to undo or else go quite counter to Sir Robert's doings.

ON TRIGONOMETRICAL FORMULÆ FOR FINDING THE LEVEL OF TWO DISTANT OBJECTS.

By R. G. Clark, C.E.

The object of this communication is to exhibit some simple formulæ, that may at one operation serve to determine the levels of distant objects with respect to the station from which the angles are taken. The subject may be considered thus: we have on a vertical plane H A D B, the given angles H A C, H A D, from H A the sensible horizon, and the given height C D, to determine the level of D at the base with respect to B, the base of the station A. (*see* fig. 1.)

There are two cases: first, when the vertex A of the station is above the level of the summit of the object C D; and, secondly, when the vertex A is below the level of C.

1st. Let the given angle of depression H A C $= \beta$; also the angle H A D $= \theta$; the given height of the object C D $= h$; and H D $= y$. Then by the triangle C A D we have

$$\sin(\theta - \beta) : h :: \sin(90^\circ - \theta) \text{ or } \cos\theta : \frac{h \cos\theta}{\sin(\theta - \beta)} = \text{A C}.$$

By the right-angled triangle A H C, we have

$$\sin\beta : y - h :: 1 : \frac{y - h}{\sin\beta} = \text{A C}.$$

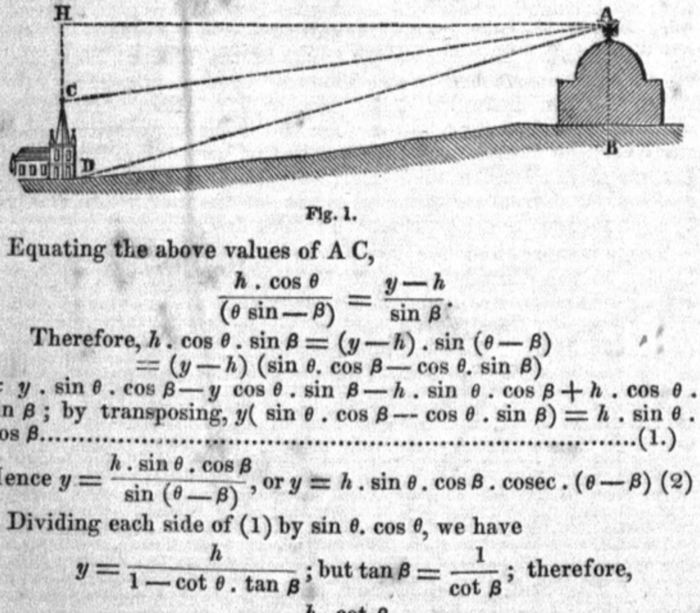


Fig. 1.

Equating the above values of A C,

$$\frac{h \,.\, \cos\theta}{(\theta \sin - \beta)} = \frac{y - h}{\sin\beta}.$$

Therefore, $h \,.\, \cos\theta \,.\, \sin\beta = (y - h) \,.\, \sin(\theta - \beta)$
$= (y - h)(\sin\theta \,.\, \cos\beta - \cos\theta \,.\, \sin\beta)$
$= y \,.\, \sin\theta \,.\, \cos\beta - y \cos\theta \,.\, \sin\beta - h \,.\, \sin\theta \,.\, \cos\beta + h \,.\, \cos\theta \,.\, \sin\beta$; by transposing, $y(\sin\theta \,.\, \cos\beta - \cos\theta \,.\, \sin\beta) = h \,.\, \sin\theta \,.\, \cos\beta$ ....(1.)

Hence $y = \dfrac{h \,.\, \sin\theta \,.\, \cos\beta}{\sin(\theta - \beta)}$, or $y = h \,.\, \sin\theta \,.\, \cos\beta \,.\, \text{cosec} \,.\, (\theta - \beta)$ (2)

Dividing each side of (1) by $\sin\theta \,.\, \cos\theta$, we have

$$y = \frac{h}{1 - \cot\theta \,.\, \tan\beta}; \text{ but } \tan\beta = \frac{1}{\cot\beta}; \text{ therefore,}$$

$$y = \frac{h \,.\, \cot\beta}{\cot\beta - \cot\theta} \text{ .... (3.)}$$

The formula (2) may be adapted for logarithmic computation, thus—

$\text{Log } y = \log\sin\theta + \log\cos\beta + \log\text{cosec}(\theta - \beta) + \log h - 30.$

The equation (3) can be effected by natural co-tangents, to be found in Hutton's "Mathematical Tables." An example is subjoined to elucidate the equation (2).

Ex. It being required to determine the level of two objects by angles of depression from the sensible horizon, taken at the summit of an edifice, as St. Paul's, the height being 404 (*see* fig. 1); the angle H A C $= 3^\circ 20'$; the angle H A D $= 8^\circ 30'$; and the height C D $= 300$ feet.

Here $\theta = 8^\circ 30'$; $\beta = 3^\circ 20'$; $\theta - \beta = 5^\circ 10'$; and $h = 300$.

By formula (2) we have

Log sin 8° 30′	9·169702
Log cos . 3° 20′	9·999265
Log cosec . 5° 10 ...	11·045501
Log 300	2·477121
	2·691589 = log H D = log 491·7.

Therefore 491·7 — 404 = 87·7 feet.

Hence B is 87·7 feet above the level of D.

2nd. When the summit of the station A is below the level of the vertex C of the object C D, fig. 2.

Let H D $= y'$; D C $= h'$; the angle H A D $= \theta$; and the angle H A C $= \beta$; and proceeding exactly the same manner as before, we have

$$y' = \frac{h' \,.\, \sin\theta \,.\, \cos\beta}{\sin(\theta + \beta)}, \text{ or } = h \,.\, \sin\theta \,.\, \cos\beta \,.\, \text{cosec}(\theta + \beta) \text{......(4.)}$$

$$\text{And } y = \frac{h \,.\, \cot\beta}{\cot\beta + \cot\theta} \text{ .... (5.)}$$

The only difference being in the signs.

Hence D C − A B = height of one object above the level of the other.

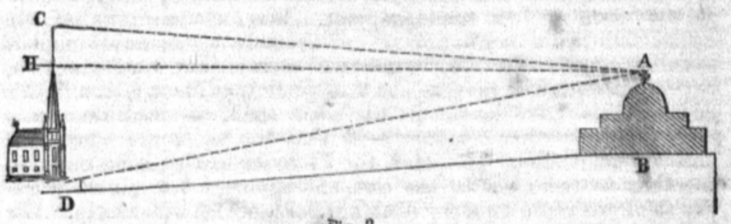


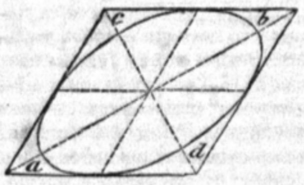
Fig. 2.

Remark. When the angles are taken to seconds, it is more advantageous to use (2) and (4), as the case may be, to ensure greater accuracy. To solve the foregoing question by the First Case of Plane Trigonometry would require thereby more than two operations: hence the manifest value of the above formulæ.

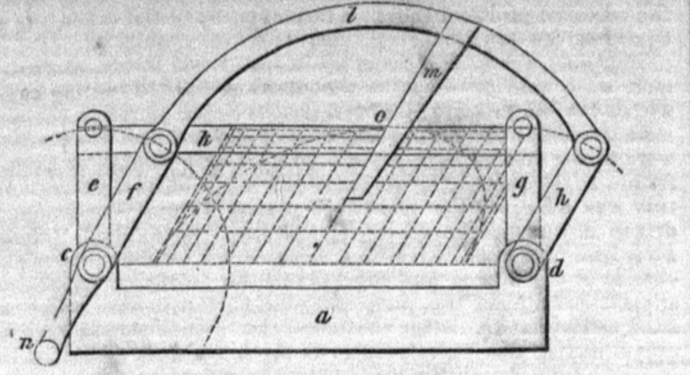
The subject is a valuable exercise to the young student in surveying, as giving him proper ideas of the utility of trigonometrical formulæ for the means of rendering operations more simple for computation.

ISOMETRICAL PERSPECTIVE.

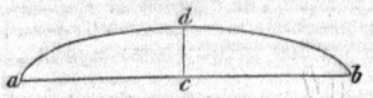
It is to be regretted that isometrical perspective is not in more general use amongst engineers and architects; but the infrequency of its application to designs of machinery and buildings, arises in a great measure, perhaps, from the difficulty usually experienced in properly representing curved lines. In this kind of perspective, we know that a circle inscribed within a square is represented by an ellipse touching the centre of each side of an oblique parallelogram, as in the annexed figure. Now, there is no instrument which can be set to describe such an ellipse, unless we first discover the transverse and conjugate diameters *a*, *b*, and *c*, *d*,—a somewhat troublesome preliminary. It is, therefore, much to be desired that an instrument could be contrived, whereby the draughtsman would be enabled to produce the ellipse with no more trouble than the simple measurement of the radius of the circle to be represented. Perhaps the following suggestion may supply that desideratum:—



To a block of metal *a*, let there be two projecting pieces, *c* and *d*, each one carrying a pair of radius-bars *e*, *f*, and *g*, *h*; let those bars exactly agree in length, and let each pair be so united by a pin passing loosely through the projecting piece, that their centre lines form an angle of 30°. Then, if *e* and *g* be made to carry a straight bar *k*, and *f* and *h* an arch *l*, having a tongue or blade *m*, which bears upon *k*, we shall have an instrument by means of which we can describe an isometrical ellipse; and the mode of using it will be to press upon *a*, with the thumb of the left hand, and with the fingers of the same hand turn the arm *m*, and thereby the two pairs of radius-bars—while at the same time the right hand holds a pencil point in the corner *o*, at the crossing of *m* and *k*. The pencil will thus be made to describe half the isometrical representation of a circle, whose radius is equal to that of the arms *e*, *f*, *g*, *h*; and by turning the instrument or the paper round, the other half may be drawn with equal facility.



By constructing the instrument so that the arms could be adjusted to any radius at pleasure, we should then be enabled to represent a circle of any diameter within its range. Ellipses, also, could be represented in this kind of perspective, by its means. If it were required to show the semi-ellipse *a*, *b*, *c*, *d*, the only adjustment necessary would be to set the radius of *f* and *h* to the line *a*, *b*, and that of *e* and *g* to the line *b*, *d*,—and the curve produced would be the true figure, according to the rules of the art. Moreover if the angles formed by the arms could be altered at pleasure, we should have at command an infinite variety of simple curves, embracing all that are derived from the isometrical projection of regular figures in any plane.



The form of the instrument may differ materially from that represented here—it may be made to describe the whole figure without being moved from its first position; but our sketch and description will, no doubt, be sufficiently suggestive of all that is required to render it efficient.

R. B. C.

PORTABLE COFFERDAM,*

SPECIALLY ADAPTED FOR HARBOUR AND OTHER MARINE WORKS.

By Thomas Stevenson, F.R.S.E., Civil Engineer, Edinburgh.

(Read before the Royal Scottish Society of Arts, January 10, 1848.)

When it is necessary, in the execution of marine works, to carry on founding or excavation in exposed situations within the high-water mark, cofferdams of the common description are not found to be answerable. Many circumstances conspire in rendering such erections inapplicable in situations where they are required to stand for several tides. The waves occasioned by a very moderate breeze of wind will, in many cases even in the course of a few hours, either entirely break up a well-constructed cofferdam, or render it leaky and unserviceable. Again, where there happens to be a covering of a few feet of sand above a rocky bottom, the piles will be found, even where there is shelter from the waves, to have no stability, and to fall inwards as the sand is removed from the interior, although every care be taken to support them with *shores* or struts.

The temporary dams which are generally employed in the execution of tide-works are of a very simple construction, and are intended to be serviceable during only one or two tides. They consist of a row of short piles which are driven in the line of a runner or waling-piece, and as the excavation proceeds, the piles are from time to time driven farther down. But this kind of erection is very unsatisfactory, and in many situations, and for a variety of purposes, it is in fact quite useless; for I have always found that it was impossible with this dam to drive the piles straight, from there being only one waling-piece to direct them. But even although they could be driven, a farther source of inconvenience still remains, for, as the stuff is removed from the interior, there is nothing left but the single waling to resist the pressure from the outside, and the bottoms of the piles being speedily forced inwards, all attempts to carry the excavation farther must necessarily be abandoned.

At Hynish harbour, Argyllshire, in 1843, I had a talus-wall to found on sand, which covered a rocky beach to the depth of from two to three feet. At another place, the rock was not only to be bared, but a navigable channel, twenty feet wide, and in some places as deep as eight feet in the rock, together with a small tide-basin, were to be excavated to the level of the low-water springs. The shores also were frequently subject, even during the summer months, to a very heavy surf.

The excavation of the tide-basin, which formed the landward part of the work, was effected by means of a series of dams, consisting of walls, built of pozzolano rubble. These were found to be quite water-tight, and to answer remarkably well in every respect; but they required, for their protection against the waves, a considerable bulwark or breakwater of *Pierres perdues* to shelter them from the waves.

In the excavation, however, which had to be undertaken seaward of the breakwater of *Pierres perdues*, any attempt to exclude the water during the whole of the tide, was what I never considered practicable. A trial was accordingly made to effect the excavation by means of a low wall, composed of a clay-rubble, resembling in its object those low dams consisting of logs of wood bedded in clay, which are often adopted in harbour-works, and which are only intended to keep out the tide during the first part of the flood, and to be pumped dry before the operations of the next tide are begun. But after many attempts with this clay-wall, it became quite evident that it would not be possible, with its assistance, to carry the excavations to near the level of low-water springs, which was due principally to two causes. First, because sand and shingle were, during almost every tide, washed in large quantities over the top of the wall into our excavation pit; and, secondly, because the waves washed out the clay from among the stones, so as to render the barrier no longer water-tight.

Being now compelled to set about some other way of carrying on the work, I had recourse to the simple method shortly to be explained, and which more than realised my expectations. Before giving a description of this method, however, it will be interesting, as well as still farther explanatory of the required objects, to quote a few lines relating to somewhat similar difficulties, from a Report upon the Harbour of Peterhead, which was drawn up in the year 1806 by the late Mr. John Rennie:—"The next material object of consideration," says the Report, "is that of deepening the harbour, which at present cannot well accommodate vessels drawing more than 12 feet of water in the spring-tides, but in neaps is not sufficient. To render this harbour more extensively useful, it would be advisable to have 17 or 18 feet of water over the greatest part of its bottom, and particularly along the west quay. The mode of performing this kind of work will be different, according to the difference of situation. Those places where the tide ebbs from the surface, and continues so for some time, may be done by blasting, or by loosening the stones with quarrying tools in the usual manner; but *in those parts where the tide seldom leaves the bottom, and in others but for a short time, different methods must be resorted to.* The best of all would be enclosing large spaces by cofferdams, and working at all times of tide by quarrying tools or blasting, as might best suit; but in some situations this would be inconvenient, as the dams would be in the way of vessels going into and coming out of the harbour. In such situations perhaps the simplest and most expeditious mode would be to use cast-iron cylinders of 7 or 8 feet diameter, having strong canvas fixed to the lower flanch, which might be kept to the bottom by bags of sand in places where there was but little agitation; but where there is much, an outer cylinder might be sunk thereon, to keep them in their situations."

The cylinders proposed by Mr. Rennie were, no doubt, quite adequate to the special purpose and locality for which they were designed, and they unquestionably possess some advantages not to be gained by other means; but, on the other hand, they are attended with difficulties and disadvantages which precluded their adoption in the present case. Those objections were the limited area, the weight and unwieldiness of such cylinders, their inflexible nature and unalterable form, as affording no means in themselves of adaptation to the very irregular rocky bottom which was to be excavated, and what was of as much consequence, the difficulty which must have attended the removal of the *partitions* of rock, or those parts which would necessarily be left between the different compartments of the cutting. The last two objections, it may be remarked, refer equally to wooden caissons, or other contrivances on the same principle.

In the present case, then, the following requisites were to be provided for. In the founding of the talus-wall, all that was required was some method which would enable the found-stones to be laid as deep in the sand as possible, for which purpose the dam did not require to be absolutely water-tight, provided it were capable of excluding from the inside the sand which was so liable to replace what was removed from the interior. For the excavation of the rock, on the other hand, it was necessary that the dam should be water-tight, and suitable for taking out all the partitions; and both situations required piles for fitting close to the irregular bottom, and those piles needed some support other than the soil into which they were to be driven.

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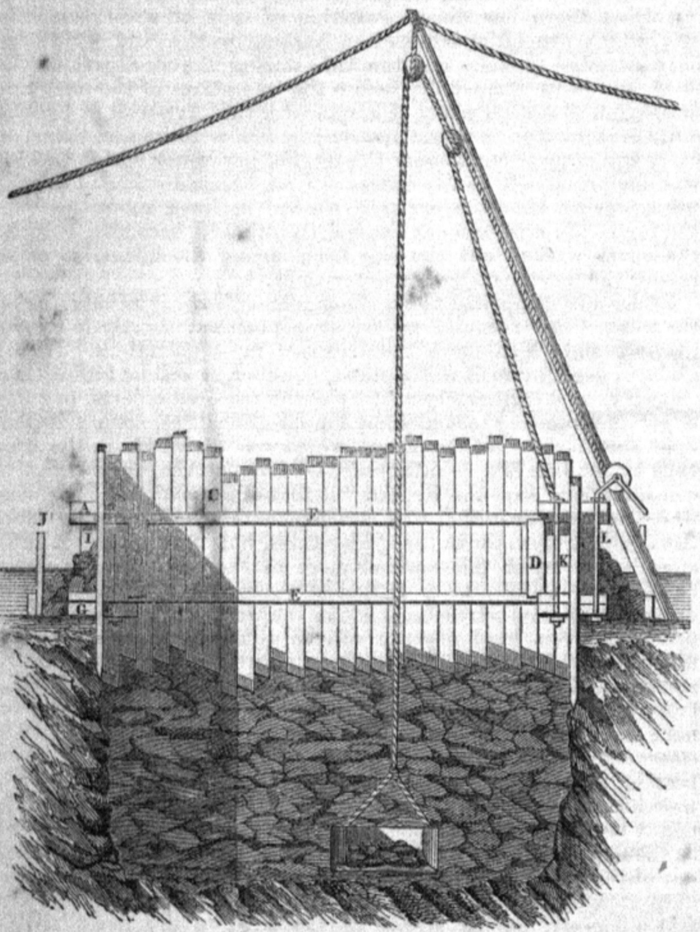
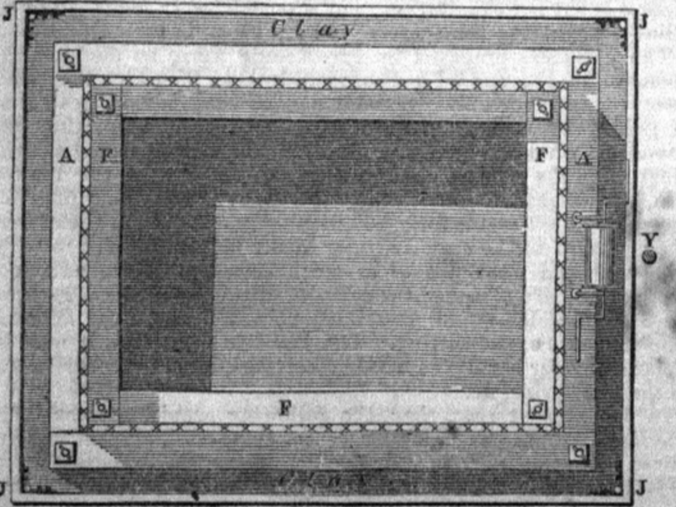
* An abstract of this paper was given in the Journal of February last, p. 62.

To effect such objects, it was clear that the means to be adopted must be at once easily managed and efficient. For although, where there is time for their employment, many complicated and troublesome refinements of construction are forced to answer purposes which might have been attained by simpler means, or by less cumbrous arrangements, yet I was well aware that in the hurry and bustle attending tidal operations and night-work, nothing can be tolerated but what is in every respect easily managed and truly efficient.

In the accompanying diagrams, A G represents a frame of double waling-pieces connected at the angles by the uprights I I, and bound together by the long bolts L, with forelocks and washers, while E F shows similar double-framed walings for the inside of the dam, and of smaller dimensions, with their uprights D, and connecting bolts K. These frames being placed in the required position, the one frame inside of the other, the piles C, are driven down between them with heavy malls.





The dam was 12 feet long by 10 feet broad inside, so that five men were able to work in the interior.* If it was to be fixed within low-water mark, the two frames being placed in the water, were guided to the spot by the men in charge, and whenever they were in the desired position, the men at once moored or fixed the frames to the bottom, by driving down a pile at each corner. After this was done, all the piles were placed between the frames and driven down, and keyed up by the small piles called "closers." Four iron jumpers J, were then driven down to their proper places outside of the frames, and edge planks for retaining the clay were slipped down upon the jumpers through iron staples, which were fixed to the planks. After this, good clay (which should have some gravel mixed with it, to protect it from the wash of the sea) was *punned* hard between the planks and the cofferdam, after which the mast N was erected, and the water taken out by means of the iron scoop shown in the drawing, which not only was used in taking out the stuff, but proved far more efficacious than any pump we ever had. Indeed, to get the dam pumped dry was for long the greatest difficulty we had to contend with. But Mr. William Downie, to whom I gave the charge, soon removed this difficulty, by using the scoop instead of a pump. The capacity of the scoop was about 37 gallons, and they generally made nine deliveries a minute, so that we found this method greatly more expeditious than any other.

As the excavation proceeded, the piles were from time to time driven down; and when the rising tide began to come over the pile-heads, or to rise above the clay, the men, before leaving their work, placed the flooring or "*deck*," as it was called, within the piling, with the ends of the planks resting upon the top of the inner frame. On this deck, ballast (consisting of stones of a convenient size) was deposited to prevent the whole frame from being floated up,—the quantity so deposited varying with the height of tide, or appearance of the weather. As each compartment of the excavation was completed, and before the dam was removed, the rock below the two rows of piles which adjoined the next cuttings was completely taken out, and the piles driven down to the bottom of the excavated pit, and left standing.† When the dam was taken up, the frames were, for the next compartment of cutting, again superimposed upon one of the rows which had been left standing in the last pit. In this way no rock could possibly escape being removed; and when the frames were to be put down anew, there was no difficulty (although the pit was entirely covered with sand) in knowing exactly the position which they were to occupy, as the piles which had been left standing were an infallible guide.

The advantages peculiar to this description of dam are its cheapness,—its portability,—its ready adaptation to a sloping, or even to a very irregular bottom,—the ease and certainty with which the partitions between the different pits are removed,—and the double-framed walings that support and direct the driving of the piles. Wherever excavations require to be made in a rocky beach, covered by a stratum of sand, however thin, there need not be any hesitation in adopting this form of dam, as there is no kind of lateral support, such as stays or shores wanted, the structure containing within itself the elements necessary for its stability. It possesses, indeed, all the properties of a caisson, and has the further advantage of accommodating itself to an irregular bottom.‡

I may observe, in conclusion, that although this form of construction is specially adapted to marine works, in the execution of which it has proved a most valuable auxiliary, the same principle might also be carried to a greater extent, and be rendered fit, with little trouble, to answer for a variety of works,—such as underfooting quay walls, founding bridges, and in removing fords or other obstructions from the beds of rivers. The application of a *double-framed waling* I have also found in itself a very useful application in several situations, and for a variety of purposes.

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* Since this paper was printed, a cofferdam on the same principle and thirty-five feet square, has been made for the Forth Navigation works, Stirling, where, in the removal of the "fords," under my direction, much difficulty has hitherto been experienced, from the constant flow of the river.

† Before lifting the cofferdam, the pit was filled with sand, to support the piles that were to remain, which, when the works were done, was cleared out by means of a water-scour, provided for the purpose of keeping permanently open the navigable tract.

‡ In situations also, where there is a considerable depth of water, and where, consequently, the frames must be made so as to stand high above the ground, it will be found of great advantage to plank the outside of the frames between A and G. This will not only make the dam more water-tight, but have the effect of binding and strengthening the framework.

THE THEATRES AND PORTICOS OF ANCIENT ROME.

Paper read at a meeting of the Royal Institute of British Architects, June 12th. By the Rev. Richard Burgess, B.D.

In looking among my antiquarian and literary stores to prepare a subject for the Institute, I again found that it was not necessary for me to go out of old Rome, for although in a series of papers spread over some twelve years, I have led you "o'er steps of broken thrones and temples," and placed you in forums, baths, or halls, I have never yet described to you the theatres and porticoes which formed so important a feature in the architectural beauties of Rome.

The amphitheatre was an edifice unknown to the Greeks, the theatre was hardly ever naturalized among the Romans, and with the exception of some tragedies, ascribed to Seneca, which are lost, it does not appear that a single Roman tragedy was ever composed upon a Roman subject. Porticoes, which were generally in the vicinity of theatres and circuses at Rome, are the natural growth of a climate subject to great heat and sudden rains: we lose in these northern regions that great ornament of a city, the portico. Our admiration is limited to arcades and covered markets, which it must be confessed are more for use than ornament. But I return to the theatre. The ancient and modern drama differ as widely as the buildings in which they were respectively acted, and I shall hardly succeed in making my Roman theatre intelligible, unless I first indicate a few of the leading features which run through the Greek drama and its Roman descendant. The subject is by far too vast and intricate for me to attempt anything like an essay upon the Greek stage, and therefore I must limit my observations to what is strictly necessary for explaining the internal arrangements of the edifice. The Greek drama dealt more in set speeches than in broken dialogues, and did not admit more than three interlocutors at once: the action or event represented was brought within the space of time in which it might in reality have been accomplished. As a general rule, there was no change of scene during the piece. In every tragedy there was a body called a *chorus*, who took no part in the action of the piece, but reflected upon what was going on, and generally expressed what might be supposed to be the sense of the audience. The chorus did not come upon the stage, but occupied the orchestra, varying the dialogue which they sometimes held with the actors by choral songs and dancing. These terms of *stage* and *orchestra* I shall shortly have to explain.

Dramatic entertainments, both in Greece and Rome, formed part of the public expenditure, or they were exhibited gratuitously by some wealthy or ambitious citizen. The theatres, therefore, were of immense size, for they were meant to contain (in Greece, at least) the whole male population of great cities. The performance usually took place also in an uncovered theatre in Greece; but Roman luxury, at a later period, invented the awning. I once described to you, when I read a paper on the Colosseum, how this awning was contrived to cover such an immense space; and I must be allowed to suppose that you have not entirely forgotten that description. If any of you are desirous of satisfying your curiosity upon the Greek stage, I must refer you to "Butenger de Theatro," for I now hasten to the buildings themselves, which it is the principal object of this paper to describe. The origin of the theatre is rather ignoble;—it was originally a wagon, in which Thespis conveyed his actors about, with their faces besmeared with lees of wine, and from which they spoke their parts to the crowd assembled around them. To the ambulatory wagon of Thespis succeeded a moveable wooden structure, which was set up and taken down at pleasure, and it was in consequence of one of these structures having given way under an unusual crowd, that the first stone theatre was erected in Greece, by Themistocles, not long after the defeat of Xerxes. From this they began to increase in number, and we have the remains of several yet existing, both in Greece and in that part of Italy which was Greek in language and customs long after it came under the Roman dominion. We have also those remains of Greek theatres in Rome, to which I shall shortly direct your attention. A theatre became so necessary an appendage to a town, that Vitruvius gives systematic directions concerning the selection of a site. In his fifth book, cap. 3, we have the following:—"When the forum is finished, a healthy situation must be sought for, wherein the theatre may be erected, to exhibit sports on the festival days of the immortal gods, for the spectators are detained in their seats by the entertainment of the games, and remaining quiet for a long time, their pores are opened and imbibe the draughts of air, which, if they come from marshy or otherwise unhealthy places, will pour injurious humour into the body. Neither must it front the south, for when the sun fills the concavity, the enclosed air, unable to escape or circulate, is heated, and then extracts and dries up the juice of the body. It is also to be carefully observed that the place be not dull, but one in which the voice may expand as clearly as possible." One cannot let pass this quotation from the great architect of the Augustan age, without remarking that the selection of a site for an important public building was considered by Vitruvius as falling within the province of the architect. A healthy place for the theatre selected, we come next to consider its shape and disposition.

The form of the Greek theatre originated, as is thought, in the natural recess of a hill-side, and most of the theatres whose vestiges I have visited in Greece, occupy that position. Mantenia, built in a marshy place, offers an exception, and I believe there is another exception in Asia Minor; but it was evidently the practice to lighten the labour of erecting such buildings by making use of a ravine, or locality adapted to the purpose. At Megalopolis I was able to trace the whole *cavea* or hollow of the theatre, partly cut out of a hill; but the seats are overgrown with thick brushwood. The same economy is observed in most of the Greek *stadia* also, and even the council of Areopagus sat on seats, cut out of, or inserted into Mars' Hill. At Nicopolis, near Prevesa, the form of the theatre on the hill-side is preserved, and much of the proscenium. At Smyrna I was able to trace the *cavea* in a similar position, and also at Ephesus we get to the slope of Mount Prion, which overlooked the Temple of Diana, in the plain of the Cayster, before we find the theatre. Whilst the Greeks, however, hewed seats out of the rock, or excavated to a depth suitable to their purpose, as the nature of the ground allowed, the Romans usually built their theatres upon arches, and massive walls rose (as we see the theatre of Marcellus still existing at Rome), with two or three orders, like the Colosseum. The hollow which perhaps originally was adjusted according to the nature of the ground, in no definite curve, ended in a perfect semicircle. This was called in Greek "κοιλον," and in Latin *cavea*, and was the part for the audience. The other part was devoted to the business of the play, and thus we arrive at the two principal parts or divisions of the theatre. The κοιλον, or *cavea*, is easily described; it was bounded by the segments of two concentric circles, the inner arc separating it from the orchestra; in the Roman theatre it seldom exceeded a semicircle, but sometimes the extremities of the semicircular arc were prolonged by straight lines; the Greeks took more of the circumference of the circle, and cut the κοιλον by lines drawn from its extremities converging towards the centre of the circle, by which arrangement more space was made in width for the *scena* or stage. The *cavea* was fitted up with rows of seats rising in succession, so as to afford each tier an uninterrupted view: the whole was divided, as in the amphitheatre, into flights by διασωματα or *præcinctiones*, which is the Vitruvian term for our landing-place. The *præcinctio* ran round the whole, and afforded an access from one flight to another. The entire arc was again cut into sub-divisions, called κερκιδες, in Latin, *cunei*, from being formed like wedges: the lines which effected those sub-divisions were called κλιμακες, or *scalæ;* these (which in the Roman circus were called *viæ*) led from the bottom to the top of the theatre, and they all converged to the centre of the orchestra. The lowest seats were considered the best, and were, in fact, the reserved seats for the magistrates and persons of office. As the audience rose in height, it descended in quality, until it reached the open portico at the very top, which has its counterpart in our shilling or sixpenny gallery. This portico, however, in an uncovered building was of some use, in confining the sound and giving shelter to the spectators from a passing storm. A κοιλον, or *cavea*, such as I have now described, would contain, in some of the largest theatres, as many as 30,000 to 40,000 spectators, which is about the capacity of those whose remains are yet to be seen in Rome. I now come to the other part of the theatre, which is more complicated and more difficult to describe. In Greek we have to deal with the three terms of ορκησρα, οκηνη, and παρασκενια. In the Roman language, we have the three corresponding terms of *orchestra*, *pulpitum* or *scena*, and *postscenium*, to which we are to add the *porticus*. I shall content myself with describing the Roman arrangements, and simply pointing out where the Greek theatre differed. Taking the *cavea* to be a semicircle, the concentric arc which separates the audience was also a semicircle, and this space, bounded by the diameter, was the *orchestra*,—not so called from anything relating to music, but because it was the place for the dancers. In the Greek theatre the segment was less than a semicircle; but if the circle be completed and a square inscribed in it, whose sides are parallel to the diameter, the side farthest from the *cavea* fixes the front of the stage; but in the Roman theatre the diameter itself determines the front of the stage, or *pulpitum.* The stage, therefore, in the Roman theatre, is brought nearer to the audience, and made deeper. The length of the stage was two diameters of the orchestra. The increased depth was rendered necessary on account of the greater number of persons assembled upon it; for the Romans put both the chorus and the musicians upon their stage. The points from whence the several staircases began to ascend the *cavea* were fixed by the vertices of four equilateral triangles, inscribed within the circle (when completed) of the orchestra. In the Roman theatre, as we have already observed, the front of the stage was called the *pulpitum;* and it was from that part that the interlocutors spoke. Some think that the *pulpitum* was a little elevated above the level of the stage; but at all events, the word has passed into use for designating a place to speak from in our sacred edifices. The lowest range of seats was raised above the area of the orchestra (one-sixth of its diameter), and the seats themselves did not exceed 1 ft. 4 in. in height. The stage in the Roman theatre was only elevated 5 feet above the seats in the orchestra: in the Greek theatre it was double that height. I have only hinted at the geometrical precision with which all these things were defined, and I shall relieve you from such dry details by a reference to the drawings behind me. The *postscenium* speaks for itself: it was a long narrow gallery behind the *scena*, where the actors retired, and where apartments or compartments were provided for them. From the *postscenium* were passages into the porticoes or gardens, which generally surrounded the theatres: but to these I shall have occasion to refer when I have finished the history and description of the theatres at Rome, to which I now come.

I have already remarked, that the earliest theatres at Rome, as well as at Athens, were but temporary erections of wood. The Romans were satisfied with standing-room for 200 years, and no seats were allowed; "lest," as Tacitus says, "if the people sat, whole days might be spent in idleness." Notwithstanding this prohibition to build permanent theatres, the temporary edifices were constructed with a magnificence which surpasses all belief. The wealth which supplied those theatrical exhibitions was generally the plunder of rich provinces: easily earned, and as easily dissipated, merely to obtain favour with the people, and procure still more lucrative appointments. All the bribery and corruption that ever came before a committee of an English House of Commons sink into insignificance compared with those times "when Rome was free." The treating of our "worthy and independent electors" at the open house of the candidate, was economy and parsimony compared with the lavish expenditure of a candidate for the honours and emoluments of a Roman governorship; and we cannot doubt, that whilst those worthy citizens were feasting for whole days at the expense of a Scaurus or a Curio, they would be loud in the praises of liberty; and had they known how to put their exclamations into the polite language of modern Europe, the air would have resounded in the midst of those entertainments with "*Vive la République!*" It was not until the year of the city 699 (that is, within 53 years of the Christian era), that a theatre of solid materials was built at Rome, and this was constructed by Pompey on his return from Asia, at the close of the Mithridatic war; but even Pompey found it expedient to pay a deference to the popular feeling. "Therefore," says Tertullian, "Pompey the Great, less great by his theatre only, when he erected that stronghold of wickedness, dreading lest the rebuke of the Censor might injure his memory, he built a temple to Venus on the top of it, and when he invited the people to come to the dedication, he did not call it a theatre, but the Temple of Venus, to which, he said, 'we have subjoined seats for seeing shows.'" The seats were therefore considered as the steps by which to ascend to the temple. We may call this either a pious fraud or a legal fiction. A piece of marble was found, in 1525, near the site of Pompey's theatre, on which Marliano read the words, "*Veneris Victricis.*" This building was erected in the third consulate of Pompey, and when the inscription came to be placed on the frieze, a dispute arose whether it should be *cos. tertio* or *tertium.* The matter was referred to Cicero, who advised the disputants to settle the controversy by writing *cos. tert.*

At the dedication of his famous theatre, Pompey produced twenty elephants; and when he was accused in the senate of introducing too much luxury into the city, he convinced the conscript fathers that it was an economy to build a solid theatre at once instead of raising a temporary structure on every occasion of giving shows. The Temple of Venus served very well as a pretext for making seats, *gradus spectaculorum;* but it could not equally be alleged for erecting a solid stage. It was not until the reign of Tiberius that this part of the theatre was added, and finally completed by Caligula. It was dedicated anew by the Emperor Claudius, who restored it after a fire, and it reached its greatest splendour in the time of Nero. Two vanquished chiefs, who came from the north of Germany to render submission to the emperor, were taken to Pompey's theatre in order that they might see the greatness of the people. It contained, according to Pliny, 40,000 spectators; and when Tiridates, king of Armenia, came to Rome, Nero caused the whole to be gilded, to show off the magnificence of the Romans to the vanquished Asiatic. It passed through a succession of events until Theodorus commissioned Symmachus to rebuild it; but not long after it shared the fate of the rest of the splendid edifices of Rome, and finally came into possession of the Ursini family, who occupied that quarter of the city in the wars of the middle ages. In the fifteenth century, an inscription, found with the name of Pompey, directed the antiquary to find out its site. Another indication of the place where this theatre stood was given in the finding of the famous statue which is now in the Palazzo Spada. That statue was found under the partition wall of a house, and lying across in such a way as to give two proprietors of the house a claim to the treasure: not able to agree about dividing the spoil, they came to the resolution of cutting Pompey in two, and each man taking his own half. The matter having reached the ears of Cardinal Capodifezzo, he hastened to Pope Julius III. to inform him of the judgment that had been pronounced upon the statue. The astonished pope dispatched a messenger with all haste, and sent 500 scudi to be divided between the litigants, instead of Pompey. Flaminius Vacca, who relates this anecdote, says the statue was found near the Palazzo della Cancelleria, in the Vicolo dei Scutari. The statue did not stand in the theatre, but in the Curia which Pompey built as an appendage to it; and the belief still obtains that it is the statue at the feet of which Cæsar fell. Being thus directed to the site of this famous building, we find ourselves in the immediate neighbourhood of the Church of St. Andrea della Valle. From near that church to the Palazzo Pio, the site is marked by a gradual rising of the ground, but no vestiges meet the eye. In order to see the remains of Pompey's theatre, we enter the court-yard of the Palazzo Pio, and descending into the vaults upon which the Palazzo is built, we find ourselves, at the depth of 40 Roman palms, among the foundation arches. These have been originally hollowed out of the natural rock, and they are pointed at the angles with large blocks of peperine stone. One of the *cunei* or sections of the *cavea* belonging to the lowest tier, may be perfectly traced; and after ascending to the court-yard again, and upon entering the stables, we see a second story of arches for supporting the seats, the construction of which is remarkable for its solidity; and it would not be difficult to trace, among the modern buildings and in the cellars of the Palazzo, at least one-half, perhaps two-thirds, of the whole *cavea.* I will not stay to describe to you the blocks of peperine and *opus reticulatum*, for the great point gained by tracing the *cunea* is the fixing of the position of the *scena* or stage. This appears to have reached very near the present site of the church of St. Andrea. But the most remarkable circumstance attending an investigation of the buildings erected by Pompey in this part of Rome, is the being able to present a ground-plan of them, although they have almost all vanished from off the face of the earth. In the sixteenth century there was found behind the church of SS. Cosma and Damiano a plan of ancient Rome, done in marble, and which had served to encrust the walls of the Temple (it is supposed) of Romulus and Remus. This marble map, where the ground-plan of all the public buildings was laid down, was found broken into fragments; some of them irrecoverable; others, gathered up with care and put together, presented an idea of a building. They now encrust the walls of the staircase of the Capitoline Museum, and are known under the designation of the Pianta Capitolina.* The two fragments most perfect happen to represent the Theatre of Pompey and the Portico of Octavia. By a reference to that fragment of the Pianta, you will not only see the ground-plan of the theatre, but also of some other buildings which were attached to it. Vitruvius cites the Porticus Pompeiana as an example of what a portico should be, when attached to a theatre for the convenience of the actors, or for the people to take shelter in, in case of rain. We know, from Martial, that Pompey's Portico had a hundred columns. Eusebius calls it, in consequence, "Hecatonstylon." The Pianta Capitolina exhibits some of those columns, but the fragment is imperfect. This celebrated portico was painted by artists of renown—Antiphilus, Pausias, and Nicias—the subjects being suited to the atmosphere which Ovid's lovers breathed. About the portico were rows of plane trees, interspersed with stone statues of beasts; and a fountain threw up, or poured out, its sparkling waters. The Pianta Capitolina exhibits two rows of columns, running in a direction towards the river, and not unlikely conducting to a grove along the banks of the Tiber. Besides these appendages to the theatre, there was the Curia, or senate-house, which is, no doubt, identical with the "Regia Theatri" of Suetonius: but I must forbear to expatiate beyond the proper limits of my subject. A careful inspection of that part of Rome where all those buildings stood, with the aid of the Pianta Capitolina and the antiquarian notices which I have cited, might still furnish a fine subject for the genius of a restoring architect; and when we consider that those extensive and magnificent buildings (whose very remains, at the end of nineteen centuries, excite our wonder) were erected out of the private resources of a single individual, it will be long before we find in another republic a popular favourite, who may vie in wealth, taste, and splendour, with citizen Pompey.

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* These fragments were first engraved and illustrated by Bellario, and are reproduced at the end of tom. iv. of the "Grævius' Roman Antiquities."

I shall pass quickly over the next theatre, which time and floods have not spared. It was erected in the twelfth year of the Christian era, by Cornelius Balbus, in compliment to Augustus, and was capable of containing 33,000 spectators. I am not aware that a vestige of this theatre remains, but Piranesi took considerable pains to ascertain the site, and found some remains of one of the *cunei*. The Palazzo, and Monte Cenci, now point to where it stood, and Camucci, one of the oldest of Roman antiquaries, who probably saw some remains of it in his time, states that from its vicinity to the Tiber, it frequently suffered from inundations. We are not aware of any portico attached to this theatre, but there was a *crypta Balbi*, which stood near it, and of this there are some remains.

The third theatre which adorned imperial Rome was that of Marcellus, and along with it I take the portico of Octavia: when I have given you some account and description of these two objects, I shall relieve you from this tedious *conversazione*.

The remains of the theatre of Marcellus are worthy of the architect's admiration. Eleven arches of both orders, and part of a twelfth, are conspicuous, though mutilated and disfigured by the dusky habitations into which they have been metamorphosed. The first order is nearly half interred, but the capitals of the Doric columns, as well as the entablature, are well preserved in several places. The second story exhibits a specimen of the Ionic order, as it was brought to perfection in the age of Augustus. Within those arches which formed the *ambulacra*, as in the amphitheatre, the *gradus spectaculorum* rose, and some of the *cunei* may be traced to the stables of the Osteria della Campana. The materials are tufo, mingled with brickwork, resembling those in Pompey's theatre, and one may perceive by a solitary column in the Via Savelli, standing at an angle with a piece of wall running in the direction of the *scena*, that the stage and its outworks must have touched the very banks of the river. The Palazzo Orsini, formerly Savelli, is built upon the ruins of the stage. Piranesi has calculated the capacity of this theatre to contain 25,000 persons: it was therefore the smallest of the three. Julius Cæsar, perhaps, laid the foundation of this edifice; but it was left for Augustus to complete it, and he dedicated it with the name of the Young Marcellus. On the feast of the dedication it is said that 700 wild beasts from Africa were consumed, and then, for the first time in Rome, there was seen a tamed tiger. We have an account of a fire having partly destroyed this theatre, but we hear very little of its history until Pierleone, in the twelfth century, made it a fortress. It passed successively into the possession of the Savelli and Orsini families, and there is no reason to suppose it has existed for several centuries otherwise than it now presents itself: in "Carnucci's Antiquities," we have a drawing of it, bearing date 1565, and it is there exhibited just as we see it at the present time. I may mention that its exterior walls are of travertine stone. The 25,000 spectators are now replaced by some workers in charcoal, and some mules, the former occupying the places reserved for the magistrates, and the mules having taken possession of what was the orchestra. Near to the theatre of Marcellus stood the famous Portico of Octavia, to which I have finally to call your attention. And although we must penetrate into the filthiest habitations in Rome, among stinking fish, in order to see the remains of this splendid work, it will amply repay us for our excursion, and stamp indelibly upon our memories the flavour of the Pescheria and the conservative habits of the Jews who live within the Portico of Octavia.

The first marble building ever erected at Rome was a portico which stood on this self-same site. It was built by Metellus Macedonicus: two temples were comprised within it. The architects were two Spartans, whose names were Sauros and Batrachus. They not only contributed their skill, but, as they were rich men, they employed their wealth also in the undertaking. The only reward for their services which they asked of the Romans was that their names might be mentioned in an inscription on the temples; but this honour being refused, they contrived to introduce their names allegorically: Sauros meaning a lizard, and Batrachus a frog—those animals were introduced into the capitals of the columns. The architects of the Portico of Octavia were also Spartans, and they respected the works of their distinguished countrymen. The new portico comprised in its circuit the two temples, made more magnificent and probably much enlarged: the fragment of the Pianta Capitolina, with the mutilated inscription "CVS OCTAVIÆ," gives us the plan of those splendid works. Bellori, in his illustration, computes about 270 columns. I confess I cannot make out that number with the utmost stretch of my feeble imagination, but I can present you with a plan made on the authority of the fragment and the ruins which still exist, adjusted on the dark tints, which show them in their proper places. By this plan it will be seen that the principal remains consist in six large columns of the vestibule or entrance (and who that has seen Rome has not admired the magnitude and elegance of those Corinthian columns), eight more columns of the exterior row of the peristyle, which are only to be seen by contending with the fish-stalls made out of the spoils of the portico; and there are also further remains of one of the temples to be seen in a Vicolo behind the church of St. Angelo: three columns standing at an angle indicate the position of the prostyle of the Temple of Juno; the other was dedicated to Jupiter. I must now leave you to raise up from this ground-plan, and from the splendid vestiges which remain, the elevations and architectural views of the portico and its temples; but even your ingenuity would not be able to restore to their proper niches, or affix on their respective walls, the works of art which once adorned the Portico of Octavia. In the Temple of Juno was her statue, made by Dionysius and Polycles; and a Venus by Philiscus of Rhodes. In the corresponding Temple of Jupiter was the much admired statue of the god, which was equally well executed by those sons of Timarchus. There was a group representing Pan and Olympius wrestling together, the work of Heliodorus; and perhaps the Venus of exquisite beauty which Pliny tells us adorned this portico (the work of Phidias) may be the very Venus de Medici which Santo Bartoli declares was found here in the Pescheria. In a part of the building called the Schola Octaviæ was the famous Cupid of Praxiteles, which called forth the eulogia of Cicero, Strabo, and Pausanias. It is more than probable that several of those statues perished in the fire which took place in the reign of Titus, and still more might be lost in that which happened under Sep. Severus. The paintings which adorned the walls and vaults were not less celebrated. There was the famous work of Artemon representing Hercules ascending from Mount Œta to Olympus, having put off his mortality with the consent of the gods; there was the painting by Antiphilus, where four figures of satyrs were grouped around the noble Hesion; and Alexander and Philip, with Minerva. You may exhibit to us an elevation restored; a few columns will direct you to complete the portico, and a medal will give you a finish for the pediment. You may square us off the basements, and crown the balustrade with colossal statues and urns; and you may festoon or triglyph the entablature;—but you cannot paint afresh the works of Artemon and Antiphilus, nor mould again the forms which the innate flash of the mind of a Phidias or a Praxiteles could produce. You must therefore be content with the bare recital, and inscribe upon the very best edifice I can describe, "*Stat nominis umbra.*" But you will remark in the enumeration of the names of those celebrated artists, that they were all foreigners, and Rome owed her most splendid works of art—I may say all of them—to the Greeks; and they owed much of their theatrical amusements to the Syrians and Egyptians. It is, in fact, a mistake to attempt to nationalise either art or science. If there be such a thing as communism in the world, it exists in the realms of genius, and no petty jealousy should ever attempt to exclude the foreigner who brings his originality of thought and genius to adorn a country of which he is not a native. It was written over the tomb of Ludlow, in Switzerland, "*Omne solum fortipatria;*" and the same may be said of the man of true genius—he belongs to every country: and I should say it generally betrays a sense of inferiority wherever there is an attempt to exclude from fair competition the genius which comes from a foreign shore. The Romans did not this, even in the histrionic art; they excelled in gladiators, but they were inferior in sculpture, and painting, and architecture: by admitting foreigners they ended by taking the lead in architecture, at least; and perhaps the studio of Emilius might have furnished a work worthy of the best Greek sculptor. And let me remark, that there are some things which no patriotism or spirit of nationality will accomplish. It will never make a man of taste admire an ordinary painting or an ungraceful statue; it will never reconcile him to a meagre elevation of an ill-designed public building.

I have generally attempted, in my papers which I have had the honour to read at the Institute, to show the influence which the public institutions under consideration might have upon the character and destiny of the Romans; and perhaps there is much more connection than at first sight appears between the works of the architect and the national character of the people. The architect is called upon to create only those works which are suitable to the habits of the people, and his object should be to study to do these well; and I, for one, do not regret that he is now compelled to study the construction of churches and schools, rather than that of theatres and porticoes. I am more than satisfied with the unarchitectural erections of Covent-Garden and Drury-Lane for theatres; and for porticoes and groves Vauxhal and the Surrey Zoological Gardens.

I am still compelled to speak only of the comparative innocence of our places of public resort; they are immeasurably inferior in architectural beauty, but they are a great improvement upon the moral aspect, and the restraints and the reflective influence of Christianity have even reached our public amusements. We cannot wonder at the indignation with which the early Christian writers viewed the theatres and places of public entertainment among the Romans, where every brutal passion or lascivious desire was gratified, and where vice in every form was enthroned by universal suffrage. We can excuse these holy men applying to these rendezvous of iniquity the title of "Devil's houses," for which I would hope no member of this Institute will ever have to give a plan. It is to my mind a happy circumstance that the *cavea* is now transferred to the lecture-room, the *orchestra* transformed into an Exeter-hall platform, the *stage* to the floor of the House of Commons, where sometimes members offer to die, and the *pulpitum* to the place from which the people are instructed in the truths and duties of Christianity. You will excuse me, then, if I rather rejoice over the ground you have lost in modern times for the exercise of your beautiful art; and that *my* profession has so amply supplied, by the sacred edifice, the field that is gone from you in the profane. I rejoice, not because either you or I have a stage more or less for our exertion, but because I think that the best interests of mankind and the happiness of the human race are more likely to be promoted by a church than by a theatre; and if we compare the national tastes of two neighbouring and rival countries in this respect, we at least shall be satisfied with the results;—and although I am loath to end this paper with a sentiment that may sound harsh to some, I cannot but be of opinion, that as the influence of Christianity prevails, and sober-minded pursuits follow as a matter of course in its train, theatrical representations, except for children, will give place: meanwhile, whatever tends to purify our places of public resort, and make them really places of recreation, is a benefit conferred on the morals of the rising generation.

THEORY OF STEAM-ENGINES.

Account of the experiments undertaken by order of the Minister of Public Works, France, upon the recommendation of the Central Committee upon Steam-Engines, to determine the principal laws and numerical data which enter into the calculation of Steam-Engines.

By M. V. Regnault.*

Introduction.—The theoretic calculation of the work done by steam-engines is founded upon some incontestable principles of general mechanics, and upon several physical laws which are far from having been, up to this time, established upon certain bases. The authors who have written upon the theory of these machines, have been obliged to admit as the basis of their calculations, laws which ought only to be considered as hypotheses to which physical philosophers have been led, most frequently, by extending to vapours, laws which are not even rigorously exact for permanent gases. Thus, when the work really done by a machine is compared with that deduced from the theory, we always find, even in the best machines, a considerable deficit. A great part of this deficit may be attributed to the disturbances produced in the physical conditions, by the very motion of the apparatus; it is due to the loss of active force *(force vive)* occasioned by the cooling of the steam; to the resistance which is developed during its course through tubes of irregular forms, and in its passage through openings, more or less contracted. Finally, there are losses of active force produced by the friction and vibration of the different pieces of which the machine is composed. But a great part of the difference may well be occasioned by the inaccuracy of the fundamental laws which have been admitted into the calculation.

Mechanics have, for a long time, greatly desired a general investigation for the purpose of establishing these fundamental laws upon a series of direct experiments executed with the means of precision which physical sciences now present. I had for some time formed the determination of devoting myself to this work, and had several times tried some introductory experiments, which however served only to show me that precise results could only be obtained by means of large apparatus, whose expense of construction far surpassed the very narrow means which we have at our disposition in our physical laboratories, and I should have been completely stopped in the execution of my projects, if the Minister of Public Works (upon the suggestion of M. Legrand, under Secretary of State), had not, with a kindness which will be appreciated by all the friends of science, placed at my disposal the funds necessary for the execution of this long and laborious work.

In order to show clearly what are the principal laws upon which the theory of steam-engines rests, it appears to me necessary to explain, in a few words, the principles of this theory. All known systems of steam-engines may be divided into four classes:

1. Engines without expansion, and without condensation.
2. Engines with expansion, and without condensation.
3. Engines without expansion, but with condensation.
4. Engines with both expansion and condensation.

The first three classes, may, in a theoretic point of view, be considered as particular cases of the fourth class, which presents the most complex case; the only one to which it is necessary for us to pay attention. We shall suppose an imaginary engine, which is not subjected to any external cause of cooling, nor to any loss of active force by friction, contractions of orifices, &c, &c. We shall suppose the boiler to be of very great capacity in comparison with the cylinder, so that the pressure of the steam may be considered as absolutely constant in the boiler during the motion of the machine; the heat of the furnace reproducing, constantly, the quantity of steam consumed by the machine.

Let ω be the surface of the piston expressed in square metres*.

x, the space described by the piston from the instant of the arrival of the steam in the cylinder, with the tension which it has in the boiler, until the moment at which we are examining it.

P, the constant pressure of the steam in the boiler, expressed in kilogrammes and referred to a square metre of surface.

T, the temperature of the steam.

v, the capacity, in cubic metres, of the part of the cylinders described by the piston from its starting point to the height, x.

V, the total capacity of the cylinder.

I. A first law, which it is important for us to know, is *the law which connects the elastic forces with the temperatures.*

We will distinguish two periods during the stroke of the piston: during the first of these the cylinder communicates freely with the boiler; the total pressure of the steam upon the surface of the piston is $P\omega$.

If the piston advances by a quantity dx, the element of work produced wil be $P\omega\,dx = P\,dv$.

The whole quantity of work produced during the first period, that is, from the beginning of the motion of the piston until the introduction of the steam is stopped (corresponding to a capacity V, described by the piston in the cylinder), is PV.

During the second period, which is that of the expansion, no more steam comes from the boiler, but the steam contained in the cylinder continues to press upon the piston; as this rises, the steam occupies a larger space, its elastic force diminishes, and its temperature is lowered by the absorption of latent heat during its dilatation.

Experiment has not decided what are the laws which govern these variations; but one of the following cases must happen:

First case.—The quantity of heat absorbed by a kilogramme of liquid water at 0° (32° Fahrenheit) in passing into vapour (which, for the sake of simplicity, we shall call *the total heat of the steam*), is the same, whatever may be the pressure, provided the vapour be at its maximum of density. If this law be exact, the steam will always remain in a state of saturation during the whole period of the expansion; the pressures of the steam will vary in the inverse ratio of its volumes, and they will constantly present the relations to the temperatures, which connect the temperatures of saturated steam with its elastic forces.

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* We are indebted to the "Franklin Journal" for the translation.

* In the following translation we have preserved the French units of length, weight, and temperature. The metre is 39·371 inches. The kilogramme 2·205 lb., av.
The degree of the Centigrade thermometer, 1·8 degrees Fahrenheit.
To reduce Centigrade to Fahrenheit degrees, multiply them by 9, divide the product by 5, and add 32 degrees.

Second case.—The total heat of the steam increases in proportion as its elastic force is greater. As we suppose that the steam is not subjected to any external cooling influence, it is evident that, in proportion as the steam dilates into a larger space, it will require a smaller quantity of total heat to keep it in the state of vapour. Consequently, during the dilatation, there will be a disengagement of a certain quantity of latent heat, which will become sensible to the thermometer, and will raise the temperature of the steam above the point which corresponds to its saturation. The temperature of the steam will then be more slowly reduced than in the former case; the steam will be found overheated during the expansion, and the pressure of the steam upon the piston will diminish more slowly than it would according to the law of Mariotte.

Third case.—The total heat of steam is less in proportion as its elastic force is greater. If this law were true, there would be a precipitation of liquid water during the expansion, the steam would remain constantly saturated, but the elastic force would decrease more rapidly than according to the law of Mariotte.

In the absence of decisive experiments to show the accuracy of one of these three hypotheses, mechanicians have generally adopted the first, which is at the same time the most simple and the most precise. This hypothesis assimilates the expansion of steam to that of a permanent gas, dilating in a variable space, whose walls constantly restore to the gas the quantity of heat which is absorbed in the latent state during its expansion, so that its temperature remains invariable.

The work developed during the expansion is then calculated in the following manner:—Let v be the volume of the steam, and p its pressure at a given moment; dx the space described by the piston while the volume becomes $v + dv$; the element of work produced will be $p\,\omega\,dx = p\,dv$. At the commencement of the expansion, the volume is V, and the pressure P, and as we admit the law of Mariotte between the volume of the steam and its elastic force during expansion, we shall have

$$p = \frac{\mathrm{PV}}{v}, = p\,dv = \mathrm{P\,V}\frac{dv}{v};$$

and the whole work produced, while the volume of the vapour passes from V to V′, is $\int_{\mathrm{V}}^{\mathrm{V}'} \mathrm{P\,V}\frac{dv}{v} = \mathrm{P\,V}\log.\frac{\mathrm{V}'}{\mathrm{V}} = \mathrm{P\,V}\log.\frac{\mathrm{P}}{\mathrm{P}'}$

This is the expression for the work produced during the period of the expansion. The total quantity produced during a complete stroke of the piston, is then

$$\mathrm{P\,V}\left(1 + \log.\frac{\mathrm{P}}{\mathrm{P}'}\right).$$

We have heretofore attended only to the pressure which is exerted upon one of the faces of the piston, but the other face is constantly submitted to the pressure which exists in the condenser. We will suppose this latter pressure to be constant during the stroke of the piston, and represent it by f. The amount of resistance which it will have produced during the stroke of the piston, will be $f\mathrm{V}_1 = f\frac{\mathrm{V\,P}}{\mathrm{P}_1}$. So that the moving power will be expressed by $\mathrm{P\,V}\left(1 + \log.\frac{\mathrm{P}}{\mathrm{P}_1} - \frac{f}{\mathrm{P}_1}\right)$.

If n represents the number of strokes of the piston per minute, the power developed during this unit of time, will be expressed by

$$n\,\mathrm{P\,V}\left(1 + \log.\frac{\mathrm{P}}{\mathrm{P}_1} - \frac{f}{\mathrm{P}_1}\right).$$

But the accuracy of the formula depends upon the accuracy of the hypothesis which we have admitted, and it is necessary to determine by direct experiments—

II. *The quantities of heat which must be given to a kilogramme of water, at 0°, to vapourize it, under different pressures.*

These quantities of heat are composed of two distinct parts—the heat necessary to raise the temperature of the liquid water from 0° to the point at which the change of state takes place, and the latent heat of vaporization. If we wish to distinguish these two parts of the total heat of steam, we must determine by experiment—

III. *The capacity for heat of water at different temperatures.*

Finally, if the total heat of steam is not constant under all pressures, in order to calculate the effect of expansion, we must still learn—

IV. *The specific heat of the vapour of water in different states of density, and at different temperatures.*

The theoretic power of a steam-engine may be estimated, by stating the amount of power which it is capable of giving for each kilogramme of steam consumed.

To do this, let ω be the weight of a cubic metre of steam under the pressure P, and at the temperature T; π the weight of steam consumed by the machine in one minute. We shall have $n\,\mathrm{V} = \frac{\pi}{\omega}$, and consequently the power given by the machine, from a kilogramme of steam, will be expressed by

$$\mathrm{P}\frac{\pi}{\omega}\left(1 + \log.\frac{\mathrm{P}}{\mathrm{P}_1} - \frac{f}{\mathrm{P}_1}\right)$$

But in order to calculate, under all circumstances, the value of ω, we must know—

V. *The law according to which the density of saturated vapour of water varies under different pressures.*

VI. *The co-efficient of dilatation of the vapour of water, in its different states of density.*

Mechanical philosophers generally admit that the weight (ω) of a cubic metre of steam, under the pressure P, and at the temperature T, may be calculated by applying to saturated steam the law of Mariotte, and the law of the uniform dilatation of gases. Now, these laws are not even rigorously exact for the permanent gases, and it is to be feared that they are completely false for saturated vapours. Finally, the method most generally adopted to compare steam-engines, consists in stating the work which they perform for each kilogramme of fuel consumed. To do this, we must know the weight (K) of steam under the pressure P, which a kilogramme of fuel can develope under the circumstances in which it is employed; and we then have, for the work performed by a kilogramme of fuel, $\mathrm{P}_1\,\mathrm{K}\frac{\pi}{\omega}\left(1 + \log.\frac{\mathrm{P}}{\mathrm{P}_1} - \frac{f}{\mathrm{P}_1}\right)$.

The quantity K, depends upon a variety of circumstances which we cannot now discuss, such as the quality of the fuel, the nature of the furnace, the arrangements of the boiler, &c.

To sum up then, the theoretic calculation of steam-engines requires the knowledge of the following laws and data:—

I. The law which connects the temperatures and elastic forces of saturated steam.

II. The quantities of heat which one kilogramme of liquid water at 0° absorbs, in being converted into saturated steam, under different pressures.

III. The quantities of heat which one kilogramme of liquid water at 0° requires to elevate its temperature to that at which it assumes the state of steam, under different pressures.

IV. The specific heat of aqueous vapour, in different states of density, and at different temperatures.

V. The law according to which the density of saturated steam varies, under different pressures.

VI. The co-efficients of dilatation of steam, at different densities.

Before commencing the search for these different laws, it was necessary to treat several preliminary questions, so as to fix with certainty the indispensable auxiliary data, and, above all, to define clearly the conditions which must be fulfilled by the thermometers, by means of which we measure the temperatures, in order that these instruments may be rigorously comparable.

These preliminary researches obliged me to undertake successively, long series of experiments, the necessity of which I was far from foreseeing when I undertook the work. I was in fact obliged to undertake the re-determination of a great number of data, which, for the most part, appeared to be fixed with complete certainty by the researches of my predecessors, and as to which physical philosophers entertained no doubts whatever.

The whole of these researches will be published in a series of detached memoirs. I intend, at the end of my labours, to sum them up in a report, which will be addressed to the Minister of Public Works, in which the results will be presented under a form suitable to the especial view with which the work was undertaken—that is, the theoretic calculation of steam-engines.

My experiments frequently required the assistance of a great number of observers. I was frequently obliged to avail myself of the kindness of several of my students, among whom it gratifies me to cite especially MM. Bertin, Grassi, Bertrand, Lissajoux, and Silberman. Let me be permitted to return to them, thus publicly, my thanks.

But I must, in a very especial manner, testify my gratitude to my friend M. Izarn, for the zeal and complete devotion with which he has aided me in this long series of labours, some of which were not without danger, and all were very troublesome, as well as in the long and tedious numerical calculations which were the consequence of them. By the aid of his active co-operation, I have been able to terminate these labours in much less time than it would have been possible for me to have done it if I had been reduced to my own personal efforts alone.

First Memoir.—ON THE DILATATION OF ELASTIC FLUIDS.

Part I.—*Dilatation of Atmospheric Air, under the ordinary Pressure of the Atmosphere.*

M. Regnault commences his memoir, by remarking that there is, in physical science, no numerical element which has been submitted to a greater number of experimental determinations than the co-efficient of dilatation of atmospheric air, and that nevertheless we cannot yet say that this co-efficient is known to us with sufficient precision. The experiments of the elder physical philosophers gave numbers so different from each other that no use can be made of them. The greater part of the circumstances which influenced the phenomenon were unknown to them.

The experiments of M. Gay Lussac (Annales de Chimie, 1st Series, tom. xliii., p. 137. Biot. Traitè de Physique, tom. i., p. 182), seemed to have settled the question finally. He showed by a great number of experiments that between 0° and 100° (32° to 212° Fahrenheit) the co-efficient of dilatation was the same for all gases, and for vapours, when they were at some distance from their point of condensation, and that its value was 0·375.*

This co-efficient was adopted by all physical philosophers, and employed in calculations, until in these latter years a Swedish philosopher, M. Rudberg, cast a doubt upon its exactness. By a series of experiments made with care, M. Rudberg endeavoured to show that the co-efficient of M. Gay Lussac was much too large, and that its true value was comprehended between 0·364 and 0·365.

The experiments of M. Rudberg are then described at length, by M. Regnault. These experiments were originally published in two memoirs contained in Poggendorff's Annals, vols. xli. and xliv., and the English reader will find them in the valuable Scientific Memoirs, edited by Richard Taylor, vol. i., pages 507 and 514.

Rudberg terminates his second memoir by an important remark, which had already been made in 1803, by Gilbert (Gilbert's Annals, vol. xiv., page 267), but had been entirely forgotten, viz: that the experiments of Messrs. Dalton and Gay Lussac, which had been regarded as having given almost identical results, differed, on the contrary, very much. In fact, in the memoir of Dalton (Memoirs Soc. Manchester, 1st Ser., Vol. v., Part 2, p. 598), he says:—

"I have repeatedly found that 1000 parts of common air, of the temperature 55°, and common pressure, expand to 1,321 parts of the thermometer; to which, adding four parts for the corresponding expansion of glass, we have 325 parts increase upon 1000 from 55° to 212°, or from 157 of the thermometer scale *(Fahrenheit)*." It is evident that the volume of air here assumed as the unit, is that of air at 55° Fahrenheit, or 12·78 cent. If, on the contrary, we take for unit the volume of air at 0° (32° Fahrenheit), and put the dilatation between 0° and 100° $= 100a$, the results of Dalton give

$$1 + 12{\cdot}78a : 1 + 100a :: 1000 : 1325; \text{ whence } 100a = 0{\cdot}392.$$

This, then, is Dalton's true result. In truth, Dalton himself does not appear to have observed the error which had slipped into his calculations, for he says in his new system of chemical philosophy:—"The volume of air, according to the experiments of M. Gay Lussac and Minè, being 1000 at 32° Fahrenheit, becomes 1376 at 212° Fahrenheit."

In a note M. Regnault notices a series of experiments upon the same subject, made about the same time with his own, by Professor Magnus of Berlin. An extract from Professor Magnus' memoir will be found in the Annales de Chimie et de Physique, 3rd Ser. tom. iv., page 330; and a second memoir upon the same subject, tom. vi., page 353.

M. Regnault then proceeds to give his own method of experimenting, and the details of his experiments.

These methods were five in number. In the first four, the dilatation of the air was deduced from the observed changes in its elastic force at the temperatures of 0° and 100° cent., assuming as true the law of Mariotte, that the elastic force of a gas varies inversely as its volume, when the temperature remains the same. The fifth method was an attempt to measure directly the augmentation of volume due to the change of temperature.

The first method was similar to that used by Rudberg, in his first series of experiments, and by Dulong and Petit, in their comparison of mercurial and air thermometers.

The apparatus consisted of a glass cylindrical reservoir, from 25 to 30 millimetres in diameter, and about 110 millimetres long, containing from 800 to 1,000 grammes of mercury. To this was soldered a capillary stem, of which the diameter varied in the different experiments, from ½ to 2 millimetres. This was bent at right angles, at some distance above the reservoir, and drawn out to a fine point. The reservoir and the greater part of the stem were immersed in a vessel of water boiling under the usual atmospheric pressure, and filled with perfectly dry air, by exhausting it from 25 to 30 times, by means of a small pump, and re-filling it each time with air which had passed through two tubes, each one metre in length, filled with pumice-stone, saturated with concentrated sulphuric acid. This being done, the apparatus was suffered to stand from half an hour to an hour, the water being maintained in full ebullition; the end of the capillary stem was then closed by the blow-pipe, and the height of the barometer noted. The reservoir, with its stem, was then inverted upon a stand, so that the point of the stem dipped to some distance in a cup of mercury, the cup was broken off under the mercury, and the reservoir surrounded with pounded ice, and left in its condition for an hour or more, until the whole of the air (now contracted so as to fill only a portion of the reservoir) was reduced to the temperature 0°. The end of the stem was then again closed by a little wax, the barometer again noted, the position of the surface of the mercury in the cup marked by a point adjusted by a screw—the cup removed, and the reservoir and its contents suffered to take the temperature of the surrounding air. The height of the mercurial column above the level of the mercury in the cup was then measured by the cathetometer. The reservoir and its contents were then weighed, entirely filled with mercury, first boiled to free it from air and moisture, the point again immersed in mercury, and the reservoir surrounded with ice. At the end of one or two hours, when it was satisfactorily ascertained that the whole apparatus had taken the temperature 0°, the ice was removed, the mercury which was discharged by the rise of temperature was received in a capsule, and the apparatus placed in a boiler, as at first, and brought to 100°. The mercury expelled was collected in the capsule, and the height of the barometer at the moment of ebullition noted. By this means, all the data necessary to calculate both the dilatation of the air, and that of the glass vessel which contained it, were given.

In performing these experiments, M. Regnault observed a serious cause of error. When the point of the stem was broken under the mercury, he observed that a small quantity of air leaked into the reservoir, even when the point was plunged to the depth of $\frac{1}{10}$ metre under the mercury. This air was a portion of that which remained in contact with the glass tube, which not being wetted by the mercury, allowed, as it were, a tube of air from the point to the surface. This difficulty was obviated by attaching to the glass stem, plates of well-cleaned brass, to which the mercury adhered, and thus the entrance of air was prevented. In addition to this, a layer of sulphuric acid was sometimes poured upon the surface of the mercury, before the point was broken, and was carefully removed before the point was again closed. Equal care was taken to prevent the air enfilming the pincers used to break the point, from getting access to the interior. In this method fourteen experiments were tried, the mean of which gives for the volume of 1,000 measures of air, at the temperature of 0°, when heated to 100°, 1·36623.

The highest number obtained in any experiment, was 1·36689
The lowest ... ... 1·36549

The difference is 0·00140

or about $\frac{1}{976}$ of the mean.

The lowest number was above the mean result obtained by Rudberg. M. Regnault believes that this may probably be due to the phenomenon of the entrance of the air upon breaking the point having taken place in the experiments of the Swedish professor, and he remarks that the error would be greater in proportion as the quantity of air operated on was less. He also states that he believes the first experiments of his own series were affected by this phenomenon, and as an evidence of this states, that from the moment that he succeeded in preventing it entirely, no experiment gave a number below 1·3659.

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* The results arrived at by Mr. Dalton, about the same time (Memoirs Lit. and Phil. Soc. of Manchester, 1st. Ser., Vol. v., part 2, p. 598), appeared to give a co-efficient, identical, or nearly so, with that of Gay Lussac, (0·3726), and confirmed his assertion as to the equal dilatation of different gases, so that Mr. Dalton himself adopted the co-efficient found by M. Gay Lussac.

The second series of experiments was tried with an apparatus differing but little from the one just described. The reservoir was a glass globe of from 350 to 400 cubic centimetres, soldered to a thermometer stem, about 38 centimetres long; upon this thermometer-tube was soldered, at the distance of 11 centimetres from the bulb, a piece of tube very regular in its diameter, about 50 millimetres long, and of a diameter sufficiently large to present but feeble capillary action. The thermometer-tube was bent at right angles, and drawn out to a point. The first operation was to gauge the apparatus carefully, and to ascertain its co-efficient of dilatation. This was done by filling it with mercury at the temperature 0°, then submitting it to a temperature of 100° collecting the mercury expelled, and weighing this, and the quantity which remained in the bulb.

The dilatation of the air was then determined very much as before. Eighteen experiments were tried in this way, the mean of all of which was 1·36633; the maximum, 1·36708; minimum, 1·36585; the difference, 0·00123, or $\frac{1}{1111}$ of the mean.

The third series of experiments was performed with an apparatus imitated from that described by Rudberg in his second memoir. Upon a shelf within a copper alembic, the cover of which is firmly fixed upon an appropriate support, is placed a glass cylindrical reservoir, 35 millimetres in diameter and 170 millimetres long; to its upper extremity is soldered a thermometer-tube, which passes through a tubulure in the cover, and bending twice at right angles, is soldered to a larger tube, which dips down into a cistern of mercury, passing air-tight through a tubulure in its cover. On the same shelf is placed a precisely similar reservoir, terminating in a straight thermometer-tube, which passes through another tubulure in the cover, and this apparatus being properly filled with mercury, furnishes a delicate thermometer for noting the temperatures in the alembic. The mercurial cistern is furnished, in its lower part, with a piston, moveable by a screw. Through a second tubulure in the cover of the cistern, passes a straight gauge-tube, open above, and dipping into the mercury below, and of the same diameter as the tube which terminates the thermometer-stem. The capacity of this apparatus having been gauged, and the co-efficient of dilatation determined by a previous experiment, the reservoir is filled with dry air, and the alembic filled with ice, so as to reduce the temperature to 0°; the piston in the mercurial cistern is then raised or lowered, until the mercury in the tube communicating with the reservoir, stands exactly at a mark previously made upon it, and the difference between this point and the top of the column of mercury in the gauge tube, is measured. The ice is then removed from the alembic, and replaced by water, which is boiled, and the temperature of the reservoir being thus brought to 100°, the piston is again adjusted, so as to bring the mercury to the same height as before in the tube communicating with the reservoir, and the differences of its height in this tube and the gauge-tube again read. These two readings of course give the elastic force of the air at these temperatures, and from these the co-efficient of dilatation is deduced. The experiments tried with this apparatus, give a mean of 1·36679—the difference between the maximum, 1·36747, and the minimum, 1·36612, being $\frac{1}{10125}$ of the mean. M. Regnault does not believe this method susceptible of the same accuracy as the other, on account of the irregular action of capillarity in the tubes, although purposely taken of equal diameters. He also remarks that the results obtained by him are larger than those got by Rudberg from a somewhat similar apparatus, which he believes may be attributed to the latter having made his mark upon a capillary tube, and to his neglecting the small quantity of air contained in the thermometer-tube, which is not heated to 100°. As however, unfortunately, M. Rudberg has not stated the dimensions of his apparatus, it cannot be ascertained what influence this had upon his results.

For the fourth series of experiments a form of apparatus was devised similar in principle to that just described, but free from its objections. This consisted essentially of a glass globe, of a capacity of from 800 to 2,000 cubic centimetres, to which was added a capillary stem about 20 centimetres long. The globe was placed in an appropriate metallic vessel, so that it could be alternately heated to 100°, and cooled to 0°; the tube passing out of a lateral opening terminated in a small copper pipe which had two other openings—one of these was for the moment closed, the other communicated with the apparatus for drying the air, by whose means the globe and tube were filled with dry air with the usual precautions. Another glass tube of 16 or 17 millimetres internal diameter was cemented at its lower end into an iron cap terminated below by a stop-cock, and carrying a lateral branch bent parallel to the axis of the tube; into this lateral branch was cemented a second tube, which was for a certain distance of the same diameter as the first, and terminated above by a capillary tube, a part of that which formed the neck of the globe, which was bent at right angles. This system of tubes being firmly and carefully adjusted in a vertical position, the second tube with its attached capillary branch was carefully dried and filled with boiled mercury, and the upper part of the capillary tube, which was of course horizontal, was then fitted into the third opening of the small copper tube, so as to be in immediate communication with the neck of the globe. When firmly fixed, the stop-cock at the bottom of the compound tube was opened, and the mercury flowing slowly out was replaced by air drawn through the drying apparatus, and the apparatus filled with air to a certain mark *a*, placed upon the vertical tube, where it was of greatest diameter, the glass globe being all the time immersed in boiling water: the drying apparatus was then removed, and the branch of the copper tube with which it communicated hermetically sealed, and the height of the barometer noted. The hot water was then discharged from around the globe, and replaced first by cold water and afterwards by pounded ice, the level of the mercury being kept at *a*, by suffering it to flow off when necessary by the stop-cock. When the globe has certainly reached the temperature of the ice, the barometer is read, and the difference of the heights of the mercury in the two communicating vertical tubes is measured. We have thus all the data for calculating the co-efficient of dilatation of the air, but another observation may be had by reversing the experiment. To do this, re-connect the drying apparatus with the copper tube, the mercury will fall in the vertical tube in connection with the globe, but must be kept at *a* by pouring mercury into the other vertical tube: when equilibrium has been attained, remove the drying apparatus, and close its branch of the copper tube, then replace the ice by boiling water, and repress the dilatation of the air by pouring more mercury into the vertical tube; when you are satisfied that the air has taken the temperature of boiling water, read the barometer, and measure the difference of the heights of the mercury in the two vertical tubes.

The mean of six experiments tried in this way gave 1·3665 for the co-efficient of dilatation: the maximum result being 1·36710; the minimum 1·36580; difference $\frac{1}{1051}$ of the mean.

By this method the dilatation of the air is determined under very different pressures; in fact, during the first period of every experiment, the air is under the atmospheric pressure 0·760 metre when at 100°, and only under the pressure of 0·550 metre when at 0°. In the second period, the air at 0°, is under the atmospheric pressure 0·760m., and when heated to 100° under the pressure of about 1·040. It is even easy to arrange the apparatus so that the experiment may be tried under still greater differences of pressure. As the experiments showed no difference in the numbers obtained during these periods (1·36655 during the first, and 1·36645 during the second period,) we must conclude that within these limits of pressure, the co-efficient of dilatation of air is sensibly constant.

Fifth series of experiments.—In all the experiments hitherto described, the dilatation of the gas was determined indirectly from a direct measurement of the augmentation of its elastic force when brought to a constant volume at a higher temperature, assuming the truth of the law of Mariotte. In order to get the dilatation directly, the gas enclosed in an eminently elastic envelope should dilate freely without changing its elastic force, and the augmentation of volume must be carefully measured, the gas being all at the same temperature. It is difficult to see how these conditions can be realised in practice, but it may be done approximately by following the method adopted by M. Pouillet in his air pyrometer. (Traitè de Physique, 4me edit. tome 1, p. 255.) In this way the elastic force of the gas remains sensibly the same, but a very notable portion in the reservoir of dilatation is at a temperature but slightly differing from that of the surrounding air.

The apparatus used by M. Regnault was to a great extent similar to that just described, but the iron cap into which the two vertical tubes were cemented was differently adjusted. It had two stop-cocks, by one of which the barometric tube could be made to communicate at pleasure with the exterior, while the other, which was placed under the tube in communication with the globe, was so bored that it might make a communication either between the two tubes or between this second vertical tube and the external air. These two tubes were placed in a glass vessel which could be filled with water so that they could be maintained at any and a uniform temperature. The experiment was conducted as follows: The globe being surrounded with ice, and the communication with the drying apparatus opened, the level of the mercury was brought to the mark *a* on the vertical tube; the communication between the two tubes being open, the mercury would of course be at the same height in both tubes; the communication with the drying apparatus was closed, the barometer and the temperature of the water around the tubes noted. The globe was then brought to 100°, the mercury in the vertical tube was of course depressed, and in order to keep that in the barometric tube at about the same level with it, its stop-cock had to be opened and the mercury suffered to flow out; the two columns were thus kept nearly at the same height, that in the tube in which the air was dilating, being brought to a second mark β, and the exact difference in the heights of the two columns was carefully noted, as well as the height of the barometer, and the temperature of the water in the surrounding vessel. In order from this experiment to determine the dilatation of the air, it is only necessary to know the capacity of the globe, and of its stem as far as the mark α, and that of the vertical tube between α and β; these are all easily determined by the weight of pure mercury necessary to fill them.

Four experiments tried in this way gave a mean dilatation of 1·36706: the maximum being 1·36718; the minimum 1·36693; difference $\frac{1}{5468}$ of the mean. The co-efficient of dilatation given by this fifth method is sensibly greater than that got from the others. This circumstance is not accidental, as in the second part of the memoir similar differences are shown for other gases, and in certain cases these differences are very considerable.*

M. Regnault then proceeds to the discussion of his formulæ, for the purpose of determining the probable error in his results, and he shows that in the first three series—principally owing to the uncertainty of the readings of the barometer within $\frac{1}{10}$ millimetre, the maximum probable error is about $\frac{1}{1000}$, which is about the greatest difference between the maximum and minimum results in any one series. The two last series include the same source of error, and another arising from the uncertainty of the temperature of the air in the capillary tube, which, however, he believes may be altogether neglected in his experiments, the apparatus having been carefully arranged so as to make this a very small fraction of the whole volume of air under experiment.

He finally assumes 0·003665 as the mean co-efficient of dilatation of dry atmospheric air as determined by the first four series of experiments, and remarks that the number 0·00367 given by the fifth series must be adopted in experiments where the gas dilates freely and preserves its original elastic force. He also gives as a fraction easily recollected, the remark of M. Babinet that 0·00366666 should be expressed by $\frac{11}{30}$.

Part II.—*On the Dilatation of some other Gases under Pressures near that of the Atmosphere.*

Physical philosophers admitted that all gases had the same co-efficient of dilatation, but since so serious an error in the numerical value of this co-efficient had been shown, it was necessary to submit this law also to verification, the result of which was to show its incorrectness. The experiments were tried chiefly by the methods I. and IV. under constant volumes, and V. under constant pressure. It is not necessary to describe them in detail, as M. Regnault has done, nor to give the methods by which the gases were purified; suffice it to say that all necessary precautions were taken, and the general results were as follows:—

Co-efficient of Dilatation from 0° to 100°.

	Under constant volume.	Under constant pressure.
Hydrogen,	0·3667	0·3661
Atmospheric Air,	0·3665	0·3670
Nitrogen,	0·3668	"
Oxide of Carbon,	0·3667	0·3669
Carbonic Acid,	0·3688	0·3710
Nitrous Oxide,	0·3676	0·3719
Sulphurous Acid,	0·3845	0·3903
Cyanogen,	0·3829	0·3877

He also describes an apparatus, an easily-imagined modification of method IV., by which the difference in the co-efficient of dilatation of any two gases may be at once shown.

Part III.—*On the Dilatation of Gases under Different Pressures.*

It has been generally admitted that the dilatation of gases is constant between the same limits of temperature, no matter to what pressure they may be submitted; consequently, that it is altogether independent of their initial density. But it is difficult to cite conclusive experiments upon which this law is founded. Several observers having obtained the same value for the co-efficient of dilatation of air, under different barometric pressures, concluded that the co-efficient of dilatation of gases was constant under all pressures; but the barometric variations in any place are not sufficiently extensive to permit so general a conclusion to be thus deduced.

Sir Humphrey Davy is the only philosopher who has studied the dilatation of gases under very different pressures. (Phil. Trans. 1823, vol. ii, p. 204.)

He states that he found the same dilatation for air taken with the densities $\frac{1}{6}$, $\frac{1}{3}$, $\frac{1}{2}$, 1, and 2; but his experiments were not made by a sufficiently delicate method to allow his results to be considered exact.

The experiments of M. Regnault upon this subject were tried with apparatus of the same character as those before described, as methods IV. and V., with such modifications as the peculiar circumstances of the experiments rendered necessary: and the conclusion at which he arrived was that "*the air dilates, within the same limits of temperature, by quantities which are greater in proportion as the density of the gas is greater: that is, in proportion as its molecules are brought nearer to each other.*"

The following tables exhibit the results of his experiments upon air, carbonic acid, and sulphurous acid:

Dilatation of Gases under different Pressures, determined by the method of Constant Volumes. (II. and IV.)

ATMOSPHERIC AIR.

Pressure at 0°. Millimetres.	Pressure at 100°. Millimetres.	Density of the air at 0°, that of air at 0°, under a pressure of 760 millimetres = 1.	Volume of the air at 100°, that at 0° = 1.
109·72	149·31	0·1444	1·36482
174·36	237·17	0·2294	1·36513
266·06	395·07	0·3501	1·36542
374·67	510·35	0·4930	1·36587
375·23	510·97	0·4937	1·36572
760·00	—	1·0000	1·36650
1678·40	2286·09	2·2084	1·36760
1692·53	2306·23	2·2270	1·36800
2144·18	2924·04	2·8213	1·36894
3655·56	4992·09	4·8100	1·37091

CARBONIC ACID.

Pressure at 0°. Millimetres.	Pressure at 100°. Millimetres.	Density of the air at 0°, that of air at 0°, under a pressure of 760 millimetres = 1.	Volume of the air at 100°, that at 0° = 1.
758·47	1034·54	1·0000	1·36856
901·09	1230·37	1·1879	1·36943
1742·73	2387·72	2·2976	1·37523
3589·07	4759·03	4·7318	1·38598

Dilatation of Gases under different Pressures, determined by the method of Constant Pressures. (V.)

Atmospheric Air.		Carbonic Acid.		Hydrogen.		Sulphurous Acid.		
Pressure Milli.	Volume at 100°.	Pressure Milli.	Volume at 100°.	Pressure Milli.	Volume at 100°.	Pressure at 0°.	Pressure at 100°.	Volume at 100°.
760	1·36706	760	1·37099	760	1·36613	760·00	760·00	1·39020
2525	1·36944	2520	1·38455	2545	1·36616	982·73	987·64	1·39804
2620	1·36964	—	—	—	—	—	—	—

The general conclusions of this memoir are as follows:—

1st. The co-efficient of dilatation of air, 0·375, heretofore admitted by philosophers from the experiments of M. Gay Lussac, is much too great for dry air under the ordinary atmospheric pressure. The co-efficient 0·3645, which is the mean of the experiments published by M. Rudberg, is too small. When the co-efficient of dilatation of air is deduced by calculation, from the changes of elastic force which the same volume of gas undergoes when carried from 0° to 100°, its value is 0·3665. But when this co-efficient is deduced from the changes of volume of the same mass of gas in passing from 0° to 100°, its elastic force remaining constant, we find a value rather higher: that is—0·3670.

2nd. The co-efficients of dilatation of the different gases are not equal, as has been hitherto admitted; they present on the contrary, notable differences, as may be seen by the numbers before cited. There is often obtained for the same gas, very different values for its co-efficient of dilatation, according as this is deduced immediately from the observation of the change of volume which the same mass of gas undergoes between 0° and 100°, its elastic force remaining the same, or calculated from the variation in the elastic force of the gas between 0° and 100°, its volume remaining constant.

3rd. The air and all other gases, except hydrogen, have greater co-efficients of dilatation in proportion as their density increases.

4th. The co-efficients of dilatation of the different gases approach nearer equality as their pressures are lighter; so that the law which is thus expressed, "*all gases have the same co-efficient dilatation,*" may be considered as a limiting law which is applicable to gases in a state of extreme dilatation; but which is farther from the truth in proportion as the gases are more compressed, or, in other words, as their molecules are brought nearer together.

(To be continued.)

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* M. Regnault describes in a note an independent series of experiments tried by him according to the method of M. Gay Lussac—that is, by observing the dilatations of a quantity of dry air contained in a true thermometer and separated from the external atmosphere by a small index of mercury. (Biot. Traitè de Phys. tom. 1, p. 182.) The results obtained did not agree at all, and were all feebler than by any of the other methods; the highest result recorded was 1·3647.

CONTRIBUTIONS TO RAILWAY STATISTICS,

IN 1846, 1847, AND 1848.—By HYDE CLARKE, Esq.

No. I.—PASSENGERS AND FARES.

Having published an analysis of the Railway Returns for 1845, I have taken the earliest opportunity after the appearance of those for 1846 and 1847, of giving a similar analysis of them, under the same title of "Contributions to Railway Statistics," which I hope may prove equally acceptable to practical men as the former series.

The following are the totals of each class of passengers in the years ending 30th June :—

	1844.	1845.	1846.	1847.
1st class,	4,875,332½	5,474,163	6,160,354½	6,572,714
2nd class,	12,235,686	14,325,825	16,931,065¾	18,699,288½
3rd class,	8,583,085½	13,135,820	18,506,527½	22,850,803½
Mixed	2,069,498½	855,445½	2,193,126	3,229,357
Altogether,	27,763,602½	33,791,253½	43,790,983¾	51,352,163

The amount received for each class, in each year, was as follows :—

	1844.	1845.	1846.	1847.
1st class,	£1,432 688	£1,516,805	£1,661,898	£1,675,759
2nd class,	1,375,679	1,598,115	1,937,946	2,048,080
3rd class,	483,069	651,903	1,032,206	1,286,710
Mixed,	147,858	209,518	93,164	146,733
Altogether,	£3,439,294	£3,976,341	£4,725,215	£5,148,002

The yearly increase in numbers on each class of passengers is as follows :—

	1845.	1846.	1847.
1st class,	12 per cent.	12 per cent.	7 per cent.
2nd class,	17 „	18 „	10 „
3rd class,	50 „	41 „	23 „
Altogether,	21 „	24 „	17 „

The yearly increase in money on each class of passengers is as follows :—

	1845.	1846.	1847.
1st class,	6 per cent.	9 per cent.	— per cent.
2nd class,	16 „	21 „	6 „
3rd class,	34 „	58 „	24 „
Altogether,	16 „	18 „	9 „

It is to be observed that no deductions can be drawn from these figures, as the Railway Department returns are defective and informal.

The gross returns in each year from passengers, goods, &c., were as follows :—

1842-3,	£4,535,189
1843-4,	5.074,674
1844-5,	6,209,714
1845-6,	7,565,569
1846-7,	8,510,886

According to Mr. Hackett, in Herapath's *Railway Journal*, the receipts for the years ending 31st December, have been as follows :

1842,	£4,341,781
1843,	4,827,655
1844,	5,584,982
1845,	6,649,224
1846,	7,664,874
1847,	8,949,681
For the year ending June 30, 1848,	9,423,963

Mr. Hackett's totals are taken from the traffic returns published in Herapath's *Journal*, and do not include many small companies which make returns to the Railway Department.

The following will show the totals of the Railway Department and of Mr. Hackett for the same period :—

	Railway Department.	Mr. Hackett.
1842-3,	£4,341,781	£4,530,401
1843-4,	5,074,674	5,114,575
1844-5,	6,209,714	6,065,956
1845-6,	7,565,569	7,159,562
1846-7,	8,510,886	8,194,767
1847-8,	——	9,423,963

Except in the first two years, it will be seen that Mr. Hackett's totals are below those of the Railway Department, for the reason already given.

1844-5,	£142,858
1845-6,	406,007
1846-7,	316,119

These figures show that any error in Mr. Hackett's figures must be on the safe side; and if we take the difference for the year 1847-8 at 300,000*l.*, this will give as the gross yearly traffic for the year ending 30th June last, 9,700,000*l.*, or nearly ten millions sterling.

The increase of passenger receipts in each year is as follows :—

1844-5	£537,047
1845-6	748,874
1846-7	422.787

The increase in the number of passengers in each year stands thus :—

1844-5	£6,027,651
1845-6	9,999,730
1846-7	7,561,180

The gross increase of revenue in each year stands thus :—

1844-5	£1,135,040
1845-6	1,355,855
1846-7	945,317
1847-8	1,200,000

Mr. Hackett has shown (Herapath's *Journal*, 3rd series, vol. X., p. 33), that the number of miles of railway on which his figures are taken, and the average traffic per mile, are as follows :—

	Miles.	Miles opened.	Traffic per mile.
1842,	1532	—	£3,036
1843,	1586	59	3,081
1844,	1780	194	3,283
1845,	2043	263	3,500
1846,	2610	593	3,288
1847,	3449	839	2,862
1847-8, (Half-year.)	3830	381	2,719

The last line has been made up from other data.

The capital expended on railways has been likewise given by Mr. Hackett, from which we can learn the amount expended in each year.

1842,	£52,380,100	whole capital,	——	expended.
1843,	57,635,100	„	£5,255,000	„
1844,	63,489,100	„	6,844,000	„
1845,	71,646,100	„	8,157,000	„
1846,	83,165,100	„	12,519,000	„
1847,	109,528,800	„	26,363,700	„

The total amount of railway expenditure from 1842 to the end of 1847 was 57,548,700*l.*

The total amount of railway income in those years has been—

1842,	£4,341,781
1843,	4,827,655
1844,	5,584,982
1845,	6,649,224
1846,	7,664,874
1847,	8,949,681
Add from Railway Returns	865,984
Altogether,	£38,884,181

Of course the whole of this income cannot be treated as real capital, no more can the whole of the expenditure; but it is a significant fact, that while the whole expenditure has been 57,548,700*l.*, the whole income has been 38,884,181*l.*, or more than two-thirds of that amount. This is deserving the attention of those who direct their attention towards the subject of railway capital.

It may be noted upon the decrease in the mileage receipts, that it is to be accounted for from the greater economy in working expenses allowing of lower fares, and from the progress of railway improvement allowing lines to be more cheaply constructed. It will be found that the net return is not less in 1847 than in 1842.

In "Irish Wants and Practical Remedies," by Humphrey Brown, Esq., M. P., (p. 63), is given a table of the estimated passenger and goods traffic of several English lines, as given before the House of Commons. This I have extended as follows :—

Name.	Length in Miles.	Passengers.	Goods. Tons.
Midland, ...	163¾	550,985	151,845
Lancashire and Yorkshire, ..	79	207,688	109,486
York and North Midland, ..	27½	185,660	5,547
London and Brighton, ..	50	226,444	43,765
London and South Eastern, ..	67	317,252	63,079
Great North of England, ..	45	75,158	32,136
Great Western, and Bristol and Exeter,	231	821,145	209,563
Lancaster and Preston, ..	20	106,957	——
Glasgow and Ayr, ..	18¾	597,470	121,027
Hull and Selby, ...	31	19,562	93,873
London and Cambridge, ..	57½	591,344	72,214
Dundee and Arbroath, ..	16½	200,727	51,899
Sheffield and Manchester, ..	44½	335,444	84,050
Altogether, ..	851¼	4,135,836	1,038,504

The traffic realised on the above lines in 1845 was as follows :—

Name.	Passengers.	Goods. Tons.
Midland,	1,809,145	715,272
Lancashire and Yorkshire,	1,674,946	507,859
York and North Midland,	464,755	351,022
London and Brighton,	788,386	65,747
London and South Eastern,	728,896	87,119
Great North of England,	196,722	234,198
Great Western, and Bristol and Exeter,	1,993,088	209,563
Lancaster and Preston,	135,344	26,099
Glasgow and Ayr,	843,078	168,376
Hull and Selby,	263,402	227,869
London and Cambridge*	534,206	44,572
Dundee and Arbroath,	269,187	81,484
Sheffield and Manchester,*	1,168,448	32,000
Altogether,	10,868,503	2,751,180

* Partially opened.

The results are as follows :—

	Passengers.	Goods. Tons.
Estimated traffic on 851 miles,	4,135,836	1,038,504
Realised traffic, 1845,	10,868,503	2,751,180
Excess over estimates,	6,732,667	1,712,676
Increase per cent.	160	170

In Mr. Brown's book on a length of 702 miles the same increase of per centage is shown, namely, 160 per cent. on passengers, and 170 on goods.

The whole traffic in 1845 was 33,791,253½ passengers, and of goods, &c. 11,600,000 tons. Supposing the proportions to be the same, the number of passengers carried in 1845 more than was provided with means of conveyance before the existence of railways was 20,800,000, and the number of tons of goods conveyed was 7,200,000. Thus the railways not only accommodated the full number of passengers for whom conveyances already existed, but carried the above enormous number in addition, besides a great quantity of goods. It will be found that this calculation is, however, far from representing the amount of accommodation now afforded.

Taking the later returns, where they are available, we shall find the increase still greater, as in 1846 for instance :—

Name.	Passengers.	Goods. Tons.
Midland,	2,468,110	900,895
Manchester and Leeds,	2,157,173	522,177
York and North Midland, and Hull and Selby,	933,514	370,414
London and Brighton,	971,081	93,407
London and South Eastern,	1,074,730	116,385
Great North of England,	239,587	433,867
Great Western, &c.	2,757,193	300,000
Lancaster and Preston,	162,012	25,585
Glasgow and Ayr,	1,091,371	293,304
London and Cambridge,	922,413	110,348
Dundee and Arbroath,	317,092	21,059
Sheffield and Manchester,	1,604,227	135,000
	13,718,503	3,530,441

In 1847 there were separate returns from some of these lines, as the following :—

Name.	Passengers.	Goods. Tons.
Great Western, &c.	2,876,222	371,326
Glasgow and Ayr,	992,096	397,515
Lancaster and Preston,	106,475	22,054
Dundee and Arbroath,	360,194	22,354
Sheffield and Manchester,	1,569,707	218,740
South Eastern,	1,477,892	204,100

The increase over the estimates on the traffic of 1846 is still greater than on 1845.

	Passengers.	Goods. Tons.
Estimated traffic,	4,135,836	1,038,504
Realised traffic, 1846,	13,718,503	3,530,441
Excess over estimates,	9,572,667	2,492,937
Increase per cent.,	230	250

The traffic on these lines stands as follows :—

	Passengers.	Goods. Tons.
Estimated	4,135,836	1,038,504
Realised, 1845,	10,868,503	2,751,180
„ 1846,	14,078,697	3,530,441

The lines for which there are separate returns in 1847 are as follows :—

Great Western, &c.,	231 miles.
Glasgow and Ayr,	18¾ „
Lancaster and Preston,	20 „
Dundee and Arbroath,	16½ „
Sheffield and Manchester,	44½ „
South Eastern,	67 „
	297¾ miles.

The traffic stands thus—

	Passengers.	Goods. Tons.
Estimated	2,378,995	529,618
Realised 1845,	5,138,041	604,641
„ 1846,	7,006,625	891,333
„ 1847,	7,382,586	1,236,081

The actual increase of traffic depends upon the length of time given for its development, beginning at 160 per cent. and going up to 230 per cent., and in the case of the selected railways even more. Taking the increased accommodation to passengers at 160 per cent., this would give the following as the increased number of travellers provided with travelling accommodation in each year :—

1844,	17,400,000
1845,	20,800,000
1846,	27,000,000
1847,	30,000,000

If the proportion be taken at 200 per cent., the number accommodated by railway for whom no accommodation was before provided, would be 34,000,000.

The following shows the proportion of traffic on railways in each year for which accommodation by coach, &c. was provided, and for which no accommodation by coach, &c. was provided :—

	Travellers from old coaches, &c.	New Travellers.
1844,	10,300,000	17,400,000
1845,	12,900,000	20,800,000
1846,	16,000,000	27,000,000
1847,	21,000,000	30,000,000

Reckoning that each passenger is on the average carried 20 miles, each male adult in this country will be carried that distance six times in the year,—an extent of accommodation which must have a great effect on trade and on the distribution of labour.

It appears from the averages given in the returns of the Railway Department, that there has been a still further reduction in fares on most of the lines, and an increase in the average speed per mile.

The total increase on each class of passengers is as follows :—

	1845.	1846.	1847.
1st class,	599,831	686,191	412,460
2nd class,	2,190,139	2,615,240	1,768,223
3rd class,	5,552,735	5,471,707	4,344,376
Altogether,	6,028,651	9,999,730	7,561,180

It is to be observed that these figures cannot be absolutely relied on, as the proportions of each class cannot be fully shown, on account of the confused state of the returns published by the Railway Department.

The total increase on each class of passengers between 1844 and 1847 has been as follows :—

1st class,	1,697,382
2nd class,	6,463,602
3rd class,	14,267,718
Altogether,	23,588,561

This is probably more than the whole traffic of the country in 1825, and it shows at any rate that there has been a great increase in the accommodation given to the working classes.

The number of first, second, and third class passengers in 1847 on the leading lines was :—

Name.	1st.	2nd.	3rd.	Total.
London and North Western,	1,112,970	3,323,380	2,163,285	5,599,736
South Eastern,	657,380	1,493,142	2,008,230	4,420,759
Midland,	445,260	1,260,312	2,571,836	4,277,419
London and Blackwall,	858,201	2,279,166	———	3,137,767
Lancashire and Yorkshire,	216,791	581,790	2,090,624	2,689,206
Great Western,	459,734	1,996,824	419,663	2,876,222
London and Brighton,	425,948	699,898	1,489,985	2,615,832
Dublin and Kingstown,	154,889	1,269,092	814,969	2,238,950
Eastern Counties,	287,526	741,486	1,044,168	2,074,170
South Western,	399,776	1,095,050	472,482	1,967,308
Manchester and Sheffield,	82,201	151,606	1,335,900	1,569,707
York and Newcastle,	152,083	753,927	643,203	1,553,213
York and North Midland,	163,837	309,782	731,207	1,204,826
Newcastle and Berwick,	67,734	174,890	944,891	1,187,515
Edinburgh and Glasgow,	105,373	206,485	836,025	1,147,883

On the London and North Western, Great Western, South Western, and York and Newcastle, the proportion of third-class passengers is much below the regular proportion.

The largest receipts from passengers in 1847 are—

London and North Western,	£1,173,798
Great Western,	674,241
Midland,	567,126
South Eastern,	335,764
Brighton,	314.493
Eastern Counties,	296,393
South Western,	286.273
Lancashire and Yorkshire,	184,762
York and North Midland,	165.434
York and Newcastle,	147,252
Edinburgh and Glasgow,	112,582

The largest amounts received for first-class passengers are—

London and North Western,	£513,795
Great Western,	232 854
Midland,	178,424
Brighton,	124,220
South Eastern,	117,659
South Western,	97,689
Eastern Counties,	93,304

The largest amounts received from third-class passengers are—

London and North Western,	£209,890
Midland,	153,354
Lancashire and Yorkshire	90,286
South Eastern,	85,403
Great Western,	77,129
Eastern Counties,	74,234
York and North Midland,	65,507

No. II.—CATTLE TRAFFIC.

The last parliamentary returns are still more defective than their predecessors, so that it is necessary to estimate some of the numbers.

The following shows the number of cattle carried in the year ending 1st July, 1846 :—

Name.	Cattle.	Sheep.	Swine.
Ardrossan,	467	3,826	306
Chester and Birkenhead,	7,508	5,461	740
Dublin and Drogheda,	429	1,186	3,636
Dundee and Arbroath	351	58	86
Eastern Counties: Cambridge,	36,238	106,055	2,613
,, Colchester,	17,134	89,211	11,190
Glasgow and Greenock,	640	1,492	—
Glasgow and Ayr,	2,136	6,567	1,424
Great North of England,	27,625	32,466	5,305
Great Western,	20,389	165,860	53,702
London and Birmingham	55,017	232,058	120,461
Grand Junction,	41,595	45,742	337,626
London and Brighton,	1,079	16,785	962
London and South Western,	6,390	62,454	5,412
Manchester and Leeds,	10,448	66,029	40,346
Maryport and Carlisle,	239	575	609
Midland, (*Estimated*),	22,000	15,000	129,000
Birmingham and Bristol,	2,641	5,274	26 044
Newcastle and Carlisle,	11,009	49.263	8,291
Newcastle and Darlington,	16,521	36,505	3,276
Newcastle and North Shields,	2,874	30.894	599
North Union	5,996	25,679	7,796
Norfolk	24,432	21,509	627
Preston and Wyre,	963	3,726	13,899
Manchester and Sheffield,	416	30,030	6,240
South Eastern,	3,892	48,344	5,224
Stockton and Darlington,	1,316	2,649	390
Stockton and Hartlepool,	302	860	420
Ulster,	999	878	27,388
Whitehaven,	15	19	—
York and North Midland,	37,657	62,249	4,944
Hull and Selby,	2,662	49,734	1,311
Total	360,314	1,209,447	813,967

As the returns are incomplete, this does not show the whole number of cattle, which will be as follows :—

Cattle,	370,000
Sheep,	1,250,000
Swine,	850,000
Total,	2,470,000

This shows an increase of 25 per cent. over the number of animals carried in 1845.

The number of calves carried in 1846 was as follows :—

Chester and Birkenhead,	6,288
Maryport and Carlisle,	1,372
North Union,	106

In other returns they are not distinguished.

The amount of revenue derived from cattle traffic was in 1846 as follows :—

	Cattle, £	Sheep, £	Swine £	Total £
Ardrossan,	30	20	6	56
Chester and Birkenhead,	237	45	12	294
Dublin and Drogheda,	95	57	180	332
Dundee and Arbroath,	28	1	1	30
Eastern Counties : Cambridge	9,864	3,693	178	13,735
,, Colchester,	2,997	2,454	239	5,690
Glasgow and Greenock,	98	24	—	122
Glasgow and Ayr,	213	135	14	362
Great North of England,	—	—	—	4 591
Great Western,	7,106	7,460	2,965	17,531
London and Birmingham,	11,715	8,817	6,161	26,693
Grand Junction,	9,126	4,000	22,365	35,491
Manchester and Birmingham	—	—	—	636
London and Brighton,	302	584	100	986
London and South Western,	1,251	2,083	479	3,813
Manchester and Leeds,	750	1,159	1,630	3,739
Maryport and Carlisle,	20	10	10	41
Midland,	—	—	—	8,960
Birmingham and Bristol,	352	151	906	1,429
Newcastle and Carlisle,	1,158	1,101	230	2,489
Newcastle and Darlington,	—	—	—	2,339
Newcastle and North Shields,	107	193	7	307
North Union,	—	—	—	20,919
Norfolk,	3,072	458	16	3,556
Preston and Wyre,	74	26	86	186
Manchester and Sheffield,	—	—	—	1,420
South Eastern	—	—	—	3,079
Stockton and Darlington,	87	32	4	123
Stockton and Hartlepool,	16	8	4	28
Ulster,	131	21	448	600
York and North Midland,	2,360	1,708	240	4,308
Hull and Selby,	793	1,491	32	2,226
Total				£167,201

On account of the very imperfect state of the returns, it is impossible to give the proportion paid in 1846 under each head of cattle traffic. In 1845 the proportions were—

Cattle,	£30,000
Sheep,	26,000
Swine,	30,000

The proportion for cattle must now be larger, and that for swine smaller.

In 1847 the number of cattle carried by each company was as follows :—

Name.	Cattle.	Sheep.	Swine.
Ardrossan,	820	332	760
Chester and Birkenhead,	1,688	6,582	1,065
Dublin and Drogheda,	680	1,794	4,338
Dundee and Arbroath,	325	32	6
Eastern Counties : Cambridge,	14,792	252,680	10,480
,, Colchester,	20,722	107,693	26,076
Eastern Union,	6,681	19,151	2,420
Ipswich and Bury,	1,408	4.848	749
East Lancashire,	287	1,290	40
Furness	3	42	—
Glasgow and Greenock	698	497	—
Glasgow and Ayr,	1,759	5,137	332
Great Southern and Western,	5,053	14,830	15,846
Great Western,	28,231	201.833	14,360
Kendal and Windermere,	108	1,814	73
Lancashire and Yorkshire, (M.&L.)	22,449	75,011	20,733
London and North Western,	161,171	399,998	150,674
London and Brighton,	2,617	28,858	3,018
Londonderry and Enniskillen,	28	108	47
London and South Western,	13,565	75,365	3,462
Manchester and Sheffield,*	6,000	5,000	10,000
Maryport and Carlisle,	924	615	2,282
Midland,*	30,000	150,000	30,000
Bristol and Birmingham,	3,526	12,771	10,684
Middlesborough and Redcar,	251	525	7
Newcastle and Carlisle,	14,599	66,628	9,759
Newcastle and Berwick,	1,908	32,224	597
North Union,	6,998	31,185	7,411
Norfolk,	38,888	85,349	8,634
Preston and Wyre,	2,245	3,788	6,169
South Eastern,	7,096	47,167	2,537
Stockton and Darlington,	1,878,	2,121	258

Estimated amount.

Shrewsbury and Chester,	336	1,269	56
South Devon, ..	222	109	6
Stockton and Hartlepool	698	2,367	592
Ulster, ..	1,273	3,313	13,360
Whitehaven, ..	34	85	——
York and Newcastle,	41,399	88,287	9,142
York and North Midland,	41,931	84,656	7,014
Total ..	483,291	1,995,354	372,987

The whole number of cattle in 1847 will therefore be as follows, allowing for the incompleteness of the returns:—

Cattle,	500,000
Sheep,	2,000,000
Swine,	390,000
Total,	2,890,000

Making nearly three million head of stock. The falling-off in swine arose from the Irish famine.

The number of calves carried in 1847 was as follows:—

Chester and Birkenhead, ..	6,534
London and South Western	9,222
Maryport and Carlisle, ..	65
South Eastern, ..	1,062
South Devon, ..	217

The amount of revenue derived from cattle traffic was in 1847 as follows:—

Name.	Cattle.	Sheep.	Swine.	Total.
Ardrossan, ..	£ 6	£ 1	£ 2	£ 9
Chester and Birkenhead,	257	54	20	331
Dublin and Drogheda,	132	65	123	317
Dundee and Arbroath,	28	——	——	29
Eastern Counties, Cambridge,	15,112	9,656	296	25,064
" Colchester,	2,949	2,710	434	6,093
Eastern Union, ..	444	158	30	632
Ipswich and Bury	102	46	15	163
East Lancashire,	11	13	1	25
Glasgow and Greenock	92	21	——	113
Glasgow and Ayr,	223	128	22	373
Great Southern and Western,	764	584	455	1,803
Great Western, ..	7,864	9,021	776	17,661
Kendal and Windermere,	1	5	——	6
Lancaster and Carlisle,	595	875	——	1,470
Lancashire and Yorkshire,	2,192	1,276	844	5,312
London and North Western,	25,435	16,622	17,223	59,280
London and Brighton,	657	880	200	1,737
London and South Western,	1,808	2,204	138	4,150
Londonderry and Enniskillen,	3	2	1	6
Manchester and Sheffield,	——	——	——	3,036
Maryport and Carlisle,	71	10	25	106
Midland, ..	——	——	——	10,270
Bristol and Birmingham,	437	380	390	1,243
Middlesborough and Redcar	9	3	——	12
Newcastle and Carlisle,	1,323	1,306	273	2,902
Newcastle and Berwick,	71	204	9	284
North Union, ..	——	——	——	15,531
Norfolk, ..	——	——	——	6,598
North British, ..	——	——	——	757
Preston and Wyre,	170	38	42	250
South Eastern, ..	——	——	——	3,334
Stockton and Darlington,	129	31	3	163
Shrewsbury and Chester,	19	20	1	40
South Devon, ..	11	2	——	13
Stockton and Hartlepool,	31	25	6	62
Ulster, ..	172	92	237	501
Whitehaven, ..	2	——	——	2
York and Newcastle,	4,255	3,925	286	8,466
York and North Midland,	2,320	2,068	1,068	5,456
		Total	..	£183,400

The total receipts for cattle traffic in each year were as follows:

1845,	£102,000
1846,	167,200
1847,	183,400

The great advance in cattle traffic was made in 1846; but the progress was not so great in 1847, as there was a positive falling-off in the number of swine carried. The greatest increase is in the conveyance of fat stock and sheep.

The following are the proportions of cattle carried in each year:—

	Cattle.	Sheep.	Swine.
1845,	236,000	1,200,000	550,000
1846,	370,000	1 250,000	850,000
1847,	500,000	2,000,000	390,000

The cattle carried to the London market in 1847, may be reckoned as follows:—

	Cattle.	Sheep.	Swine.
London and North Western,	65,000	200,000	55,000
Great Western, ..	20,000	150,000	10,000
South Western, ..	13,000	75,000	3,000
South Eastern, ..	7,000	40,000	2 500
Eastern Counties: Cambridge,	10,000	200,000	10.000
" Colchester,	15,000	75,000	20 000
Brighton, ..	2,500	25,000	3,000
Total,	132,500	765,000	103,600

The number of cattle sold in Smithfield in 1846 was 213,525, and of sheep 1,527,220, so that the railways must have engrossed a considerable part of the cattle traffic. For the conveyance of cattle to the London market the railway companies receive at least £75,000.

Great reductions have been made in the charges for the conveyance of cattle since 1845. The charges are as follows:—

		Cattle.	Sheep.	Swine.
		d.	*d.*	*d.*
London and North Western,	1845,	1·020	·160	·160
"	1847,	·626	·135	·145
Eastern Counties: Cambridge,	1845,	·980	·200	·400
"	1847,	·943	·143	·107
Eastern Counties: Colchester,	1845,	·980	·200	·400
"	1847,	·890	·153	·173
Great Western, ..	1845,	1·530	·156	·188
"	1847,	·819	·156	·188
York and North Midland,	1845,	·500	·200	·500
"	1847,	·500	·200	·500
Lancashire and Yorkshire,	1845,	·870	·250	·250
"	1847,	·766	·177	·373
London and South Western,	1845,	1·750	·200	·500
"	1847,	1·460	·160	·190

No reduction has taken place on the York and North Midland Railway, because the rates were already low.

The largest cattle traffics in 1846 were as follow:—

	Cattle.	Sheep.	Swine.
London and North Western,	96,612	277,800	358,087
Eastern Counties: Norfolk and Eastern Union, ..	77,804	216,775	14.430
Great Western, ..	20,389	165,860	53,702
York and North Midland, and Hull and Selby ..	40,319	109,992	5,225
Great North of England,	27.625	32,466	3 300
Lancashire and Yorkshire,	10,448	66,029	40,346
South Western, ..	6.390	62,454	5,412
North Union, ..	5,996	25,679	7,796
Newcastle and Carlisle	11,009	49,263	8,291
South Eastern, ..	3.892	48,344	5,224
Newcastle and Darlington,	16,521	36,505	3,376
Manchester and Sheffield,	416	30,030	6,240

The gross amounts received in 1846 for cattle traffic range as follows:—

London and North Western,	£62,820
Eastern Counties, &c.,	28,971
North Union ..	20,919
Great Western, ..	17,531
Midland, ..	8,960
York and North Midland, &c.	6,534
Great North of England,	4,591
London and South Western,	3,813
Lancashire and Yorkshire,	3,739
South Eastern, ..	3,079

The largest cattle traffics in 1847 were as follows:—

	Cattle.	Sheep.	Swine.
London and North Western,	161,171	399.998	150.674
Eastern Counties, ..	82,491	469,721	48,359
Great Western, ..	28,231	201,901	14.360
York and North Midland	41,931	84,656	7,014
York and Newcastle, ..	41,399	88,287	9,142
Lancashire and Yorkshire (M & L)	22,449	75,011	20,733
South Western, ..	13,565	75,365	3,462
Newcastle and Carlisle ..	14,599	66,628	9,759
North Union, ..	5,996	25,679	7,796
South Eastern, ..	3,592	48.344	5,224
London and Brighton, ..	2,617	28,858	3,018
Newcastle and Berwick,	1,908	32,224	597

The following will show the progress of the cattle traffic of the principal companies:—

London and North Western,	Cattle.	Sheep.	Swine.
1845,	61,466	229,245	315,989
1846,	96.612	277,800	358,087
1847,	161,171	399,998	150,674

Eastern Counties,	Cattle.	Sheep.	Swine.
1845,	20,661	125,564	4,228
1846,	77.804	216,775	14.430
1847,	82,491	469,721	48.359
Great Western,	Cattle.	Sheep.	Swine.
1845,	14,058	172,264	52.443
1846,	20,389	165,860	53,702
1847,	28,231	201,901	14,360
York and North Midland,	Cattle.	Sheep.	Swine.
1845,	15,364	88,143	31.708
1846,	40,319	109,992	5,255
1847,	41,931	84,656	7,014
York and Newcastle,	Cattle.	Sheep.	Swine.
1845,	19,685	20 000	5,000
1846,	44,146	68,971	5,531
1847,	41,399	88,287	9.142
Lancashire and Yorkshire,	Cattle	Sheep.	Swine.
1845,	9,686	149,022	27,485
1846,	10,448	66,029	40,346
1847,	22,449	75.011	20,733
South Western,	Cattle.	Sheep.	Swine.
1845,	2,763	53,441	3.089
1846,	6,390	62.454	5,412
1847,	13.565	75,365	3,462
Newcastle and Carlisle,	Cattle.	Sheep.	Swine.
1845,	3.782	37,525	5,116
1846,	11,009	49,263	8,291
1847,	14,599	66,628	9,759

The Belgian cattle traffic from the returns was as follows:—

	Cattle.	Sheep & Swine.
1843,	8.609	33,562
1844,	12,691	39,056
1845,	7,597	29,704

Taking the saving by conveyance of cattle on railways at 10 lb. per quarter, 2 lb. for sheep, and 5 lb. for swine; or 40 lb. per beast, 8 lb. for sheep, and 20 lb. for swine, the gross saving in 1846 will be—

On 370,000 cattle,	14,800,000 lb.
1,250,000 sheep,	10,000 000
850,000 swine,	17,000,000
Total,	41,800,000 lb.

The gross saving of animal food on the cattle conveyed by railway in 1847 was as follows:—

On 500,000 cattle,	20,000.000 lb.
2,000,000 sheep,	16,000,000
390,000 swine,	7,800,000
Total,	43,800,000 lb.

In the late report on Smithfield market, some evidence is given bearing on the question of the conveyance of cattle by railway:—

Mr. R. HEALY said that there is a much greater quantity of dead meat brought to the London markets in consequence of railway communication. By means of the railways, great quantities of hind-quarters of mutton are sent up from the country, as the butchers there kill large quantities of sheep and sell the fore-quarters at home amongst the population there, and send the hind quarters by railway to London.

Mr. LANGHAM, a butcher, said that country-killed meat is better than town-killed meat, and that it comes in excellent condition from Scotland. It is the general opinion of butchers that this is the case.

Mr. HICKS, the salesman, said that he has a very large quantity of meat sent up from the country by railway, and that it is not damaged by the journey even in hot weather. He has used the electric telegraph to obtain a supply of meat from the country. A communication was sent the same night by the country grazier that he would send up 600 or 700 stone of meat by the next morning's train. At 1 o'clock in the morning it started from Ipswich, and before 5 o'clock it was in his premises in Newgate market on sale, having been alive the day before. Mr. Hicks has sometimes 300 carcases on a Monday.

Mr. LANGHAM likewise stated, that since the railways have been opened a country trade in meat has been growing up. Beasts have been sent from Smithfield to Liverpool, and he has seen immense quantities of meat going down to Birmingham. The south country also is supplied from the London market with beef—Brighton in particular. The Brighton butchers are frequently seen in Smithfield purchasing cattle, which they take down with them the same day. Sometimes as many as 300 or 400 beasts have gone down by the Birmingham railway on a Monday.

These facts will show the nature of cattle traffic on railways.

THE "WESTMINSTER REVIEW," No. XCVII.:

THE NEW HOUSES OF PARLIAMENT.

Although political topics and subjects of a grave utilitarian cast form the staple of this periodical, with only occasionally an article of a lighter cast, the "*Westminster*" has in its time, and especially under its present editor, contributed more largely to architectural information and criticism than either of its rivals. In fact, the "*Edinburgh*" has scarcely once, during the whole of its long career, touched upon aught connected with architecture. One prevalent fault in *Review* articles of the kind, is the dull and impertinent prosing with which they are eked out, in order to fill up a printed sheet, or as much more as may be the space allowed, although all that the writer has to communicate would perhaps occupy not more than a couple of pages. In the present instance, we have no such complaint to make: the writer comes at once to the point, and criticises in succession (besides the New Houses of Parliament) the New Treasury Buildings, Buckingham Palace as altered by Mr. Blore, the British Museum, and the Royal Exchange; and his remarks are upon the whole so good, as far as they go, although we do not subscribe our own opinion to every one of them, that we wish he had entered more into particulars with regard to the three last-mentioned structures. How they and the "Houses" themselves are spoken of, except as regards ability on the part of the writer, we have not yet said. With respect to the Palace, indeed, it may be taken for granted that his opinion is anything but favourable, that unhappy building being abandoned to universal derision; but the writer is severe upon the others also—and not least of all, or rather more especially so, upon the Houses of Parliament, which proves that he does not take his cue from the vulgar flatteries of the public press, heaped upon Mr. Barry and his "great work." In short, he expresses himself exceedingly dissatisfied with that edifice; nor is he by many the only one who is so, for even among our own acquaintance—those, too, whose judgment in matters of architecture is entitled to some deference—we have heard opinions equally strong in disfavour of it. One serious complaint alleged against it is, that however well the florid and exuberant embellishment bestowed on the river-front may shine or sound in description, or show itself in an elevation drawing, it is all but entirely lost in the building itself;—that there is abundant sculptural decoration of some sort or other may be seen, but it cannot be at all made out. The decoration is, besides, not only too minute, considering the vast extent of the river-front, and the distance of the nearest accessible point from which it can be seen by the public, but is also so profuse, as quite to destroy "repose." While this is to be regretted for artistic reasons, it is also to be condemned for financial ones; an immense expenditure having being incurred for mere ornament, to scarcely any purpose at all. Surely the water-side of the building might very properly have been made some degrees less ornate than the other fronts, and still have been sufficiently finished-up, and sufficiently dignified and imposing,—nay, even more effective in its *ensemble* than it now is. Hitherto, stingy parsimoniousness has been allowed to betray itself more or less in nearly all our public buildings, where the effect of what is perhaps a handsome façade in itself is sadly marred by the meanness of plain brick walls, shabby chimneys, and other eye-sores that come into sight in every angular view of the building,—as is most offensively the case in the new façade of the British Museum, notwithstanding that it is decked-out in Ionic pomp, or what is meant for such. In the Houses of Parliament, the architect has fallen into the contrary extreme of error; and anxious to avoid the reproach of parsimoniousness, has incurred that of extravagance.

Besides wasteful excess of decoration, the writer in the "*Westminster*" urges against the "Houses" what he considers two capital and now irremediable defects; one of them being the want of greater loftiness in the river-front, more especially as the situation itself is very low; the other, the position of the Victoria Tower. No doubt, when all the towers in the rear of the river-front shall come to be completed, and the sheds, coffer-dam and other obstructions are cleared away, some expression of loftiness, as well as variety of outline, will be imparted to the general *ensemble*; but then that will again be counteracted by the much greater loftiness and bulk of the Victoria Tower. If exigences of plan required that the royal entrance should be just at the south-west corner of the pile,—if it was impossible to bring in that entrance as the central feature of the west side—or perhaps the east one, by forming a commodious carriage approach to it along the terrace—there was at all events no imperious necessity for carrying up such an enormous tower over it as is now intended to be done. It is true, in many mediæval edifices which have grown up by degrees, and consist of parts added to the original plan at long intervals of time, very great incongruities and discordant contrasts may be found, and may be pleaded by some as sufficient *precedent*. But the "Houses" will have been erected from one comprehensive original plan, laid down by the architect from the very outset; and so far from aiming at variety and contrasts in his elevations, Mr. Barry has most studiously attended to perfect regularity of composition and uniformity of features,—at least, such is the case with regard to the river-front, which, although a secondary one in regard to its situation, will hardly be secondary in regard to display. Nevertheless, so lofty a structure raised at one corner of the general mass as the Victoria Tower will be, must inevitably show itself as a striking irregularity,—an architectural excrescence, and apparently an after-thought—at least to those who may not happen to know that it was so planned from the very first.

With regard to the river-front, the question now is: How can it be rendered accessible to the public, so that its elaborate ornamentation can be fairly seen and enjoyed? At no very great distance of time, perhaps, and owing to the very insecure condition in which it now is, Westminster Bridge will be taken down, and either considerably lowered or rebuilt further off from the Houses of Parliament; in which case, Bridge-street will be converted into a *cul-de-sac*, similarly to the streets which run from the south-side of the Strand down to the river; consequently, the "Houses" will no longer be looked down upon from the bridge—but then how is their river-front to be looked at at all, except from a boat on the river itself? The only way of enabling the public to contemplate that façade, will be to form a second terrace or quay for foot passengers, advanced about fifty feet into the river, and perhaps about a couple of feet lower than the terrace between the advanced extremities of the building itself. Unless something of that kind were to be done, quite as much would be lost as gained by the removal of the present bridge; to say nothing of the great inconvenience attending the disturbing such a long-established line of communication and traffic.

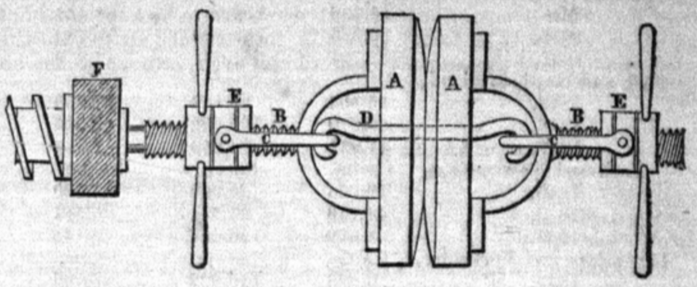
That we agree with the *Westminster*" in much or most of what it says, both in regard to the Houses of Parliament and the other structures which it notices, we freely admit. And it is pleasant to us to find opinions that are upon the whole in accordance with our own entertained by others. Speaking of Buckingham Palace, the "*Westminster*" observes that the Marble Arch "might have been advantageously incorporated with the design by an artist of resource and genius;" and again, of the British Museum, that the central portico or octastyle ought to have been loftier than the other colonnades, both which ideas have been brought forward in sketches in this very *Journal*. Although we should not have been displeased at his noticing that circumstance, supposing him to have been aware of it, we do not accuse the writer of making use of "our thunder;" on the contrary, we are right glad to meet with the coincidence of opinion, and to find that we are not altogethe solitary in our own. Here we will conclude, by earnestly recommending a perusal of the article in the "*Westminster*" to our readers. It certainly bears rather severely upon Mr. Barry, but he, if any one at all, can very well bear on his part to hear unpalatable truths. Of flattery and adulation he gets enough, or more than enough—more than may be altogether wholesome for him. An occasional draught of "bitters" will therefore do him no harm.—The dose of criticism in that review is said to be concocted and administered by a noble peer.

REGISTER OF NEW PATENTS.

RAILWAY LOCOMOTION.

Richard Wrighton, of Lower Brook-street, Grosvenor-square, for "*Improvements in apparatus to be applied to railway carriages and engines.*"—Granted December 22, 1847; Enrolled June 22, 1848.

The apparatus consists of five distinct applications to railway carriages and engines for different purposes. The first part consists in the construction of apparatus attached to the breaks of railway carriages; the actuating force upon the breaks being that of steam. The patentee claims under this head of his specification, the combination and arrangement of apparatus, whereby the piston of a steam-cylinder may be made to act upon central traction-rods or shafts, for the purpose of working the breaks of carriages and causing them to act simultaneously upon the wheels throughout the whole train; also the construction of box-coupling for connecting the shafts, and the power to the breaks.—The second improvement consists in the employment, in railway carriages, of one central buffer in lieu of the two side ones, as hitherto used; this the patentee constructs in combination with the draw-link. The annexed diagram represents this arrangement. A, A, are the buffer-plates of adjoining carriages; they are attached to the buffer-rods B, B, the ends of which are made of the looped form shown, for the purpose of admitting the loop of the connecting-links C, C. The buffer-plates A, A, are made with holes through their centres, through which are passed the double-ended hooks D, when the carriages are required to be connected, which are hooked to the links C, C, upon the buffer-rods B, B. Immediately behind the loop are cut threads, upon which work the nuts, E, E, upon which are loose collars, that do not revolve with the nuts. To the loose collars are attached by studs the links C, C; When it is required to connect two carriages, the buffer-plates are brought together, the two nuts are turned up to the looped ends of the buffer-rods, and the double hook inserted and hooked on to the links; the nuts are then to be turned back until the links and the hooks become tight. Instead of passing the traction-rods entirely through the carriage as hitherto, the patentee passes the rod B, only through the end-frames F, F, of the carriage, where the helical spring G, is placed upon the rods, and acts as the buffing-spring.—The patentee claims as his third improvement, the employment of helical springs or other elastic substance, incombination with adjusting-screws, for making the couplings of railway carriages; also the combination of a double-joint with the adjusting-screws.—The patentee's fourth improvement consists in constructing the axle-box and the axle in such manner that the lubricating material employed shall be retained in contact with the journal and the bearing, and thereby prevent a considerable portion of the waste which has hitherto taken place with axle-boxes as usually constructed. He constructs the axle of one piece or of two pieces, as in the usual way; the brass forming the bearing is properly fitted in, and the end of the axle is inserted into the axle-box from the back; the axle inside against the journal is turned with a flat or taper shoulder, against which is fitted and placed a metal ring; between the ring and the back of the axle-box is inserted a ring of vulcanized india-rubber, or other similar substance, thereby preventing the escape from the axle-box of any considerable portion of the lubricating material employed.—The fifth improvement consists of a means of enabling any of the passengers in railway carriages of a train to signal and communicate with the engine-driver or guard, by the aid of electricity. To the middle partition of each railway carriage, near the roof, is fixed a small voltaic battery, by means of which the passengers are enabled to bring into action an electro-magnet, that explodes a percussion-cap or rings a bell.



COKE OVENS.

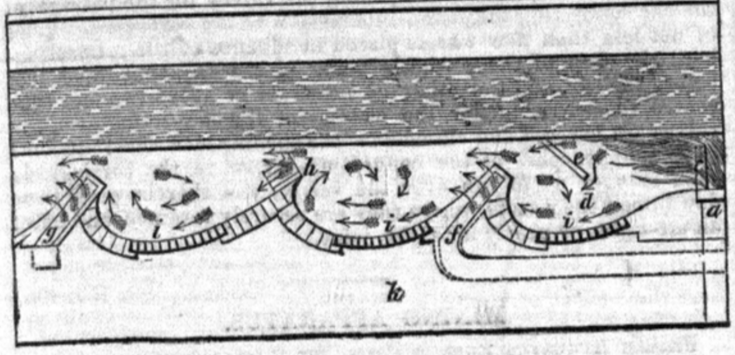
George Ambroise Michaut, of Epieds, France, for "*Improvements in the production and application of heat, and in the manufacture of coke.*"—Granted December 15, 1847; Enrolled June 15, 1848.

The application of the heat evolved during the formation of coke is the object of this invention. Several ovens are combined together, which are provided with fire-bars that occupy only a small portion of the area of the bottom. The ovens are separated from each other by partitions, and in the upper parts there are openings to permit the flame and products of combustion to pass from one oven to another; and there are openings in the top of each oven, through which the flame and heated gases ascend, in order to heat gas-retorts, lime-kilns, steam or other boilers, or other apparatus, situated above the coke-ovens. The patentee states, that by means of this invention the heat may be more advantageously obtained and applied than before. When the ovens are at work, the charges of coal are introduced in succession, in such manner that the charge in two out of three may be in a high state of ignition when fresh coal is put into the other. The operator will know when to withdraw a charge, by the flame ceasing on the surface; as soon as this is the case, the charge is to be withdrawn and cooled with water; a fresh charge of coal is then to be put into the oven, and, the fire-door being closed, the charge will soon be ignited by the heat of that oven and the flame from the other ovens. The ash-pit doors are to be kept closed at all times, except it be found requisite to introduce air to expedite combustion; and the ash-pit door of the oven, into which the air is to be admitted, is opened.

STEAM-BOILER FURNACES.

Henry F. Baker, of Boston, United States of America, for a "*certain new and useful improvement in steam-boiler furnaces.*"—Granted December 22, 1847; Enrolled June 22, 1848.

The annexed diagram represents a longitudinal vertical section of the furnace, as applied to the cylindrical boiler of a high-pressure engine. The fire-box *a*, is placed at one end of the boiler. At the end of the fire-bars there is placed a vertical grate *c*, which reaches nearly to the bottom of the boiler, for the purpose of preventing ashes from being carried into the reverberatory chambers; in the addition of which, the peculiarity of the invention consists. The bottoms of these chambers are placed below the level of the fire-bars, and the number of them may be four or more as required. They are of a parabolic form, for giving a revolving motion to the gases and other inflammable matters, thereby retaining them till consumed; *e* is a plate of iron placed in an inclined position, whereby it receives the direct force of the entire flame from the furnace, part of which is deflected against the bottom of the boiler, while the remainder is turned downwards, and caused to circulate in the chamber below. Openings are left at the upper part of the plate *e*, between it and the bottom of the boiler, which allows the flame to pass along; *f* and *g* are air-channels, for the admission of atmospheric air or oxygen, in order to ensure the perfect combustion of the gases. These channels are carried into the brickwork at the side of the boiler, through which they may be conducted, and furnished with valves, if necessary, for regulating the quantity of air; *h*, *h*, are curved plates, which extend across the furnace, the openings in which are so arranged that, while part of the heat and flame passes along immediately in contact with the boiler, part also is deflected against the bottom of the boiler, and the remainder is directed down into the hollow bottoms of the reverberatory chambers. Gratings *i*, *i*, *i*, are placed in the bottom of each of the reverberatory chambers, to allow any small particles of incombustible material to escape, that may be carried over by the draught. These gratings open into a pit *k*, below, which must be kept closed. The fire having been ignited, the flame and gases evolved will be carried over and through the vertical bars *c*, and will come in contact with the plate *e*, by which they will be deflected against ths boiler, and also turned downwards into the hollow bottom of the first chamber *d*, causing them to rovolve and be retained a short time, to further the combustion. They then pass over the upper part of the air-distributing box, on the further side of which are numbers of small openings, whence the atmospheric air issues, as indicated by the arrows, the current of air or oxygen causing the unconsumed volatile product to be converted into flame, which, by the force of the current, impinges on the bottom of the boiler. The gases are also in this next chamber caused to revolve by the curved shape; and they are disturbed by the plate *h*, in the next chamber, where they are again retained a short time, the revolving in these chambers causing the heated particles to be brought in contact with the boiler, the last reverberatory chamber being also furnished with an air-distributing box *g*, which is supplied by channels through the brickwork. From the fourth chamber the incombustible product is conducted by a flue at the bottom of the boiler to the chimney.



AXLE GUARDS AND BUFFERS.

Charles de Bergue, of Arthur-street West, City, engineer, for "*Improvements in carriages used on railways.*"—Granted January 5; Enrolled July 5, 1848.

These improvements in railway carriages relate only to the axle-guards and boxes, and to buffers. The patentee constructs his axle-guards in such a manner, that a wooden surface shall be presented as a guard for the axle-box (which is of cast-iron) to rub against. For this purpose he secures two uprights to the main framing of the carriage, at a distance from each other suitable for receiving the axle-box, which has a flange, its whole depth on each side embracing both sides of the uprights. These wood uprights forming the guard, are strengthened by plates of iron, placed on each side, the width of such plates being less by the breadth of flange on the axle-box, than the wood against which they are bolted; thus the face of the axle-box slides flush with the face of the axle-guard. The upper parts of these plates are carried up the side of the frame, to which they are secured. The springs are of the kind previously patented, and consist of a series of india-rubber rings, separated by plates of metal. This spring is placed between the uprights of the axle-guard, the bottom plate being supported by a vertical rod resting in a step on the axle-box, immediately above the centre of the bearing, being within the grease-box, which forms part of, and is cast in one piece with, the axle-box. The wood faces of the axle-guards and the chase in the sides of the axle-boxes must be rendered particularly smooth, in order to prevent abrasion of the surface of the wood. The second improvement consists of a mode of making the conical centres of plates used for separating the rings of india-rubber in the buffer and other springs. In making these plates the patentee forms a thin disc of metal of the required diameter, having a hole in the centre larger than is necessary, to go over the buffer-rod; he then incloses this plate in a suitable mould, in which is poured a quantity of soft metal, such as zinc and tin, in order to form the conical centre of such separating-plates. The soft metal mould, being larger in diameter than the hole in the centre of the plate, it consequently becomes securely imbedded in the soft metal centre, which is cast with an opening suitable to receive the buffer-rod or other spindle, according to the purpose it may be intended for. The third part of this invention relates to what the patentee denominates a long-range buffing apparatus, which is applied to a van or truck placed between the engine and tender and the train of carriages, in order to protect the train as much as possible from violent concussion. The range of resistance in this apparatus is to the extent of several yards, and, unlike other buffers, it is not attended by any recoil. The resistance is produced by the friction of straps passing over a drum, which straps are so arranged in connection with levers and a train of wheels, that as the force of the collision increases, the straps are tightened and the resistance is augmented.

SCREW PROPELLERS AND PUMPS.

Edward Humphreys, of Holland-street, Surrey, engineer, for "*certain improvements in steam-engines, and in engines or apparatus for raising, exhausting, and forcing liquids.*"—Granted January 4; Enrolled July 4, 1848.

The improvements in steam-engines have reference solely to the mode of driving the screw propellers of steam-boats. The apparatus and the driving multiple wheels are so arranged, that the cranks usually employed are dispensed with, the place being supplied by the driving spur-wheels themselves. The pins to which the connecting-rods are attached, and which have hitherto been fixed to the cranks upon the ends of the shaft, are, by the patentee's arrangement, now fixed to the bosses of the driving-wheels, which take the places of the cranks. The second part of the invention consists in the construction of the valves for pumps for raising or lifting or forcing water, as applied to an air-pump. The patentee proposes to form the passages through the bucket, radiating from the centre; the passages being in pairs, having a thin partition of metal between them, each pair of the passages being covered by a valve, which is composed of a thin piece of steel or other flexible metal, one end of which is firmly secured to the inner part of the bucket, while the other part rises from its seat, when required to allow the flow of water. The patentee claims in respect to the first part of his invention—First, the placing the direct-acting marine steam-engines between a line drawn through the centre of the piston-rod, and a parallel line drawn through the adjoing bearing, or in the spaces usually occupied by the crank in ordinary engines. Also the connecting the the piston-rods to the driving-wheels, without the aid of cranks.

LAP-WELDED IRON TUBES.

Job Cutler, of Birmingham, civil engineer, for "*certain improvements in welded iron pipes or tubes to be used as the flues of steam-boilers.*"—Granted January 13; Enrolled July 13, 1848.

The object of the patentee is to produce lap-welded iron tubes or pipes, so formed as to give increased strength to those parts which are exposed to wear, without additional weight to the entire length of the tube, and thereby to obviate the evils to which boiler tubes are at present exposed. He makes the internal diameter of the tube greater at one end than at the other, instead of its being the same, or uniform throughout, as has hitherto been the case; the external diameter remaining, however, the same, and uniform throughout the entire length of the tube. The tube will, of course, be cylindrical upon the exterior, and conical upon the interior surface. The increased thickness of metal at the one end is to be drawn from the remaining portion of the entire length of the tube. And further, the operation is effected at one heat, so that the ductility of the iron of which the tubes are composed shall remain unimpaired.

The *modus operandi* is as follows:—The patentee employs a series of grooved rolls, moved by suitable toothed wheels and a mandril, with a conical bulb or head, the stem of which is of increasing diameter towards the opposite end. The skelp, after being properly prepared, as is usual in the manufacture of lap-welded iron tubes, is heated and passed between the first of the series of rolls. It is then welded over the conical bulb, and forced, at the same time, over the stem of the mandril. This mandril is held by a grip, attached by a hinge thereto in a stop, so as to allow of its being lowered and passed, after the conical bulb has been removed between the second series of rolls, the diameter of the groove of which is smaller than that of the first series. The tube, with the mandril still inside, is then passed through the third series of rolls, the groove of which is smaller than that of the second series. The object of these successive rollings, after the skelp has been welded on the mandril, is to remove any irregularities upon either of the surfaces, and to make the edges of the tube perfectly smooth and uniform. The tube is then taken to the drawing bench, in front of which is a stop, and against which the pipe rests. The stop is furnished with a hole to allow of the passage of the grip of the mandril, which is held by a pair of pliers; and, the bench being made to move while the pipe remains stationary, the mandril is withdrawn. When it happens that the mandril adheres too tightly to the tube, it is proposed to heat it in a muffle or furnace, then to cool the end which rests against the stop, and repeat the above operation, or to roll it cold between three rollers, as is usually done in straightening shafting.

ATMOSPHERIC RAILWAY.

William Froude, of Darlington, Devon, civil engineer, for "*Improvements in the valves used in closing the tubes of atmospheric railways.*"—Granted January 5; Enrolled July 5, 1848.

The material which is employed in this invention for closing the slit in the tube, is vulcanized india-rubber; and the advantage proposed to be gained is the dispensing with any unctious substance for keeping the valve air-tight. Flat valve-seats are formed on each side of the slit, both of which are bounded by vertical flanges; the right-angles formed by the vertical flanges and the valve-seats, being rounded off, and the valve-seats slightly recessed by shallow circular recesses, which thereby form the hinges or centres of motion of the valves. There are two valves employed, the lower portion is composed of plates of iron, of about eight inches in length each plate, the shape of the lower surface of which agrees in contour with the seat upon which it is placed; the one edge of the plates forming, with the shallow circular recess in the seat, the hinge or centre of motion. The other edge of the plates is nearly over the centre of the line of opening, thereby nearly meeting the edge of the opposite plate upon the other valve-seat. The under side of the plates over the aperture, is formed of the same curvature as the inside of the tube. The upper surfaces of these plates are flat, except that part over the aperture immediately adjoining the edge, where it is lower than the part over the seat. Upon these plates are placed a continued sheet of vulcanized india-rubber which extends from beyond the centre of the line of opening or aperture over the whole surface of the plates up the inner side, and also on the top of the vertical flange. The portion of the vulcanized india-rubber sheets which are over the line of opening, are increased in thickness and fill the whole of the depressed part of the plates, thereby forming at that part a thick pad. The upper surface of the vulcanized india-rubber is covered with canvas. Above thevulcanized india-rubber sheets are placed flat plates of iron, of similar length to those beneath and lying over them; they are securely rivetted together, thereby holding firmly between them the vulcanized india-rubber. The vulcanized india-rubber is secured to the side and top of the vertical flange by means of a series of iron bars, which are bolted to the flange, and which are to be about 15 feet long, which secures that edge of the valve air-tight, or nearly so. When the valves are firmly pressed upon their seats, the edges of the vulcanized india-rubber pads in contact are below a line drawn between the centres of motion of the two valves, whereby the tendency of the elasticity of the pads will be to further press the valves upon the seat instead of raising them therefrom. The patentee proposes to employ, for the purpose of opening the valves for the passage of the bar connecting the piston apparatus with the carriages, a series of not less than five wheels placed in advance of the connecting-bar; the first of these wheels being placed at about nine feet before the bar, and the other at intervals of about two feet from centre to centre. After the passing of the connecting-arm, the valves are lowered and closed over the opening by a wheel attached to the upper part of the connecting-bar, or to the carriage, in such manner as to run over the valves, and thereby press them down upon their seats, where they are securely retained, and effect an air-tight, or nearly air-tight, joint.

MINING APPARATUS.

Pierre Augustus Puis, of Paris, for "*Improvements in apparatus for raising and lowering heavy bodies in mines.*"—Granted December 22, 1847; Enrolled June 22, 1848.

The principal feature in this invention is the application of atmospheric pressure to the raising of weights, and to the draining of mines. The first plan by which the patentee proposes to raise bodies is by having a vertical air-tight tube in which there is a solid piston, to the under part of which the weights to be raised are to be attached; and the upper part of the tube being exhausted, the atmospheric pressure below will force up the piston and its load. In the drawings attached to the specification, two pistons are represented, the one following the other in the ascent. When the upper one reaches the top it is relieved of its load by means of a slide, which passes in below it, cutting off communication with the rest of the tube, and the weight is removed by a door in the side of the tube; the upper piston is then carried by exhaustion in a continuation of the tube above the shaft. The next one is then brought up and unloaded in a similar manner. The bottom of the tube is closed after the weights are introduced, the air being admitted by a small tube proceeding from the top of the mine, by which the admission of the air to the underside of the pistons is regulated. In another method of applying the atmospheric tube, instead of raising the weights within the tube, they are elevated on the outside, by means of arms projecting through a continuous slit. For the purpose of raising water in mines, the apparatus consists of a series of air-cylinders, which are placed at regular intervals down the shaft. Each of these is in communication with an exhaust tube. The lower pump draws the first lift by suction, the water then passes through the bucket, and is forced up a step higher to a small reservoir placed for the purpose. The next pump above repeats the operation, drawing the water from the reservoir, to which it has been previously raised by the pump below, and so on till the water arrives at the top. The action of the pumps is produced by alternately exhausting and admitting the air from, and to, the cylinders, on the upper side of the pistons. Another part of this invention relates to the raising weights in mines by a series of vertical rods, which are attached to each other, forming one continuous rod to the bottom of the pit. Two of these combined rods are placed side by side, and suspended at the top by two chains, attached to, and passing round, wheels supported over the mouth of the shaft. These wheels have a semi-rotary movement imparted to them from a steam-engine, by which means a continual reciprocating motion of the two rods is kept up. Hooks or notches are formed on the vertical rods at regular distances, corresponding with the height the rod is lifted at each movement. Each bucket to be raised by these rods is furnished with two spring-hooks, which take into the notches on the rods and suspend them while ascending; the bucket being suspended from the lowest notch, is elevated by the first movement, till the spring-hook slips into the second notch on the other rod. The rod by which it has been previously lifted, descends, thereby transferring the whole weight to the other rod, which immediately raises it another step, the changes being performed alternately in this way until the bucket reaches the top.

STEAM-ENGINES.

Charles William Siemens, of Manchester, for "*Improvements in engines to be worked by steam and other fluids.*"—Granted December 22, 1847; Enrolled June 22, 1848.

The chief object of this invention is the saving of fuel by a means proposed for regenerating the steam and for condensing it by successive exposures to water of different temperatures. There is no regular boiler employed, but the steam is generated in the first instance in a small flat chamber, placed close to the cylinder; the direct action of the fire being against a cast-iron chamber containing the bottom of the cylinder, and having considerable space between the two, which is filled with lead. The flues are continued twice round the cylinder, and then carried under the steam-generating chamber. The cylinder is fitted with a jacket, through which the steam has access to both sides of the piston, the area of the upper side of which being diminished one-half by a trunk which works through a stuffing-box in the top, and admits of the connecting-rod passing down to the piston, the difference of the area causing the piston to ascend. The steam having been thus admitted to the cylinder, is suffered to escape by a series of double-beat valves, which are lifted by a series of cams on the main shaft; the steam is thus admitted successively into eight different regenerating-chambers, which are placed immediately under the generator. The steam, as admitted to the upper chamber, will be of the greatest pressure, and the valve is suffered to remain open a very short time, the next in succession opening immediately, the duration of which will be somewhat longer, which goes on increasing, the pressure of the steam being also gradually reduced; the ninth valve opens to the atmosphere or to the condenser, and remains open till the engine is past the centre. These several chambers are each fitted with shallow horizontal trays, into which water escapes from the generator above, passing from one to the other. The heat thus communicated to these chambers regenerates the steam, which is again admitted to the same cylinder, or to another cylinder, should it be a double-cylinder engine, by the same set of valves, which are opened by another set of cams on the opposite side of the shaft, in the inverse order to that before explained, the lowest pressure being allowed to act first. The heat communicated by the fire to the cylinder also assists in the regeneration of the steam. The next part of the invention relates to the condensation of steam in ordinary condensing-engines. The condenser is subdivided by horizontal partitions, into four or five separate compartments, the steam being admitted by a cock having a hollow plug, which is opened to the cylinders. This plug opening to the separate compartments of the condenser in succession, beginning at the lowest, the injection water is admitted to the uppermost, where it completes the condensation of the steam, and from which the air-pump exhausts a portion of the injection; and the condensed water instead of being carried off by the air-pump, is received between two pistons working in a barrel between the upper and second chamber, by reason of the upper piston being above the top of the cylinder or barrel. This water is by the down stroke of the air-pump admitted to the chamber immediately below the injection chamber, and in which a portion of the steam is suffered to escape. Each of the partitions is fitted with similar apparatus for transmitting the condensed water from one chamber to the other, till it reaches the lowest, where the steam is first admitted. A portion of the steam on entering that chamber will be condensed by the water, and after having passed through all the separate chambers, and having the heat derived from several successive charges of steam from the cylinder, it will have attained a great heat, and may be introduced to the boiler. The quantity of water required for injection will thus be materially reduced, and may render it applicable to locomotives for condensing the atmosphere of the steam which remains in the cylinder after its high-pressure effect is spent through the blast-pipe in the chimney. The last part of the invention relates to an improvement in the chronometric governor, patented by Mr. Siemens, in December, 1845. The improvement consists in the adaptation of an expanding fly-wheel to the governor, instead of the pendulum-ball with which it was originally constructed. This fly-wheel is formed in four segments, and by centrifugal action they are caused to expand or recede from the centre. When any excess of the centrifugal force takes place, by reason of the increased velocity, bell-crank levers are actuated by the segments, so as to force a conical friction-break against a cone fixed to the frame-work. The friction caused by this break tends to retard the velocity of the shaft and segments, and the power which is exerted to drive the shaft being constant and independent of the velocity of the prime mover, limits the velocity of the expanding-wheel, causing nearly a uniformity of motion, if sufficient power be at all times transmitted to the shaft.

REPLY TO THE REVIEW OF DR. GREGORY'S "MATHEMATICS FOR PRACTICAL MEN."

Sir—In the criticism upon the new edition of Dr. Gregory's "Mathematics for Practical Men," which appeared in the last number of your *Journal*, the writer has in such positive terms denounced as incorrect certain portions of that work, involving principles not however peculiar to itself, but which have originated with, or been demonstrated by, all the principal mathematical writers, that I think myself called upon to offer the following remarks, to prevent your readers being misled upon the subject.

Your reviewer commences his criticism by expressing his surprise at my using the words *cycloid* and *trochoid* synonymously; I am not, however, the only person who has done so, for

Dr. Hutton says: "Trochoid is the same curve as what is more usually called the cycloid."[1]

Professor Barlow says: "Trochoid is the same as cycloid, that term being derived in a similar manner from χυχλος, a *circle*. It is, however, by *some* authors, used to denote exclusively the prolate cycloid."[2]

Your reviewer then proceeds to find fault with my definition of an epicycloid, which is as follows:—

"If the generating circle, instead of rolling along a straight line, is made to roll upon the circumference of another circle, the curve described by any point in its circumference is called an *epicycloid*." p. 179. Dr. Hutton says: "But if, instead of the right line, the circle roll along the circumference of another circle, *either equal to the former or not*, then the curve described by any point in its circumference is what is called the *epicycloid*."[3]	Your reviewer says: "It is *not* called an epicycloid, except when the generating circle is equal to the fixed circle, and rolls on the exterior of it."

Now, Professor Barlow[4] gives the properties of epicycloids when the generating and quiescent circles are not *commensurable*, which they always would be if equal and Dr. Young,[5] in an "Essay on Cycloids" (the attentive perusal of which I would recommend to your reviewer), speaks of epicycloids in which, while one circle remains constant, the other becomes either infinite or evanescent. If, then, your reviewer is *right*, it follows that Dr. Hutton, Professor Barlow, Dr. Young, and many others (whom I have not space to quote), are *wrong*.

He then goes on to say, that I have aggravated the mistake "by representing the rolling curve as much *larger* than the fixed curve," whereas it is really much *smaller* in both my figures (157 and 158); from which it is very evident that he has mistaken the fixed for the rolling circle.

Your reviewer next finds fault with one of my definitions, as being "clumsy and incomplete, at the least;" and asks, "What will our mathematical readers say of such a definition?" Now, one of our most justly-esteemed mechanical writers, Professor Moseley, has given a definition so precisely synonymous, that I transcribe both his and my own for the purpose of comparison:—

Professor Moseley's Definition.	*Definition Criticised.*
"When more forces than one are applied to a body, and their respective tendencies to communicate motion to it *counteract one another, so that the body remains at rest*, these forces are said to be in *equilibrium*, and are then called pressures."[6]	"When the forces that act upon a body, *destroy or annihilate each other's operation, so that the body remains quiescent*, they are said to be in *equilibrium*, and are then called *pressures*." p. 187.

Your reviewer objects to the sense in which I here use the word

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[1] Phil. and Math. Dictionary, vol. 2. p. 541.

[2] New Math. and Phil. Dictionary,—Art. "Trochoid."

[3] Phil. and Math. Dictionary, vol. 1, p. 475.

[4] New Math. and Phil. Dictionary,—Art. "Epicycloid."

[5] A course of Lectures on Nat. Phil., vol. 2, p. 556.

[6] Mechanical Principles of Engineering and Arch., p. 1.

"pressures;" for which, however, in addition to that of Professor Moseley, I have the authority of Dr. Whewell,[7] who says, "*Statical* forces are called *pressures;*" and Dr. Young,[8] who says, "A *pressure* is a force *counteracted by another force, so that no motion is produced.*" If, then, your reviewer is *correct*, it follows that Professor Moseley, Dr. Whewell, and Dr. Young are *wrong*.

Your reviewer next takes up the subject of *vis viva*, on which I say,—

> "*Vis viva*, or living force, a term *used by Leibnitz* to denote the *force* or power of a body in motion; or the force which would be required to bring it to a state of rest." p. 108
>
> Professor Barlow says: "*Vis viva*, or living force, is *used by the same author* [Leibnitz] to denote the *force* or power of a body in motion."[9]
>
> Dr. Hutton says: "*Vis mortua*, and *Vis viva*, are terms *used by Leibnitz* and his followers for *force;* understanding by the latter, that *force* or power of acting which resides in a body in motion."[10]

Notwithstanding, however, the united testimony of Professor Barlow, Dr. Gregory, and Dr. Hutton, that Leibnitz used the term *vis viva* as here stated, your reviewer is perfectly sceptical upon the point, and boldly asserts "that Leibnitz never did anything *half so absurd* as is here said of him;" that he did do so, is however a matter of *fact*, for here are his own words:—

> "Hinc *Vis* quoque duplex: alia elementaris, quio et *mortuam* apello, quia in ea nondum existit motus, sed tantùm sollicitatis ad motum, qualis est globi in tubo, aut lapidis in funda, etiam dum adhuc vinculo tenetur; alia verò vis ordinaria est, cum motu actuali conjuncta, quam voco *vivam*. Et vis mortuæ quidem exemplum est ipsa vis centrifuga, itemque vis gravitatis, seu centripeta; vis etiam qua elastrum tensum se restituere incipit. Sed in percussione, quæ nascitur a gravi jam aliquamdiu cadente, aut ab arcu se aliquamdiu restituente, aut a simili causa vis est viva, ex infinitis vis mortuæ impressionibus continuatis nata. Et hoc est quod *Galilæus* voluit, cum œnigmatica loquendi ratione percussionis vim infinitam dixit, scilicit, si cum simplice gravitatis nisu comparetur. Etsi autem impetus cum vi viva semper sit conjunctus, differre tamen hœc duo, infra ostendetur."[11]

Not content, however, with denying that Leibnitz said that, which his own works prove that he did say, your reviewer denies that *vis viva* is a force at all, and says that it is a mere technical term; Dr. Whewell,[12] however, says, "The *vis viva* of a body in motion *is a force;*" and Professor Moseley,[13] "That the difference between the aggregate work of the accelerating *forces* of the system, and that of the *retarding forces*, is equal to one-half the *vis viva* accumulated or lost in the system." Therefore, either your reviewer is wrong, or else both Dr. Whewell and Professor Moseley.

He next states that, "in the second problem of the chapter on Statics, the calculation respecting the strain on tie-beams and struts is totally erroneous;" to which I answer that the calculation *is correct*, and that your reviewer *is wrong*; as he will find if he refers either to Tredgold,[14] Dr. Whewell,[15] Professor Moseley,[16] or Professor Wallace,[17] *(who quotes from Dr. Gregory the very problem denounced as incorrect).*

Your reviewer then extracts the following proposition relating to the centre of gravity:—

"If the particles or bodies of any system be moving uniformly and rectilineally, with any velocities and directions whatever, the centre of gravity is either at rest, or moves uniformly in a right line" p. 193 Emerson says: "If two or more bodies move uniformly in any given directions, their common centre of gravity will either be at rest, or move uniformly in a right line."[18] Dr. Whewell says: "If there be several bodies, which either all attract and are attracted by a single body, or all attract each other, these also will move in such a manner that the common centre of gravity will either remain at rest, or move uniformly in a straight line."[19]	Your reviewer merely says: "This is not true."

He then asks a question; "Does the author mean to assert, that if two bodies be moving with different [uniform] velocities in straight lines perpendicular to each other, the common centre of gravity moves in a straight line?" To which I answer very decidedly, "Yes, I do; and if you are at all sceptical upon the point, if you refer to Emerson's 'Principles of Mechanics,' p. 67, you will find the truth of my answer *demonstrated*."

The next objection of your reviewer is to my use of the term, line of rupture; which, however, I prefer to apply to the actual case of rupture of the ground, which takes place when the wall falls, and which is then obviously the same as the *natural slope:* the line determining the wedge of maximum pressure is only an imaginary line, and not that on which the ground would really separate.

Your reviewer has quite misunderstood Dr. Gregory, when he asserts, that the conditions upon which he examines the stability of an arch, are "that there are only two joints of rupture, equidistant from the crown, the loading symmetrical, and the piers incapable of sliding," no such conditions being assumed, or indeed necessary.

The next paragraph of your reviewer requires no comment from me; the obvious mis-quotation of my words does as much violence to common-sense and grammatical construction, as it exhibits the desire to pervert the meaning of what I actually say.

He then states that I have given certain experiments (which he extracts) "as the foundation of dynamics" and "in place of an enunciation of the three laws of motion," *which is not the fact:* I have merely employed them to illustrate the necessity of regarding time in estimating the forces of moving bodies: *see* Atwood,[20] Barlow,[21] and Hutton.[22]

Your reviewer next finds fault with my using the expression, "Each particle of matter *resists motion;* is he aware that Dr. Whewell[23] repeatedly uses a similar expression—"the inertia of the particles to *resist the communication of motion;*" and that M. Poncelet, in the introduction to his *Mécanique Industrielle*, has revived the term *vis inertiæ*, and has associated with it the definitive idea "of a force of resistance opposed to the acceleration or the retardation of a body's motion."

Your reviewer next takes objection to the assertion, that (neglecting the effects of friction) if a body suspended from a fixed point by a flexible string, have its path altered by a projecting pin, it will rise to the same height as it would have done if not so interfered with; Dr. Young, however, speaking on this subject says: "We may alter the form of the path in which it descends, by placing pins at different points, so as to interfere with the thread that supports the ball, and to form in succession temporary centres of motion; *and we shall find in all cases, that the body ascends to a height equal to that from which it descended, with a small deduction on account of friction.*"[24]

After stating that Dr. Gregory's definition of the centre of gyration is "confused and inaccurate," although identical (as he will find by reference) with that given by Dr. Hutton,[25] by Emerson,[26] and by Professor Barlow,[27] he proceeds to show by reference *to the "fable of the wolf and the lamb,"* that one of the propositions relating to the centre of gyration cannot be correct, and arrives at the *certain* conclusion that the author (as also Emerson, from whom the same proposition is taken) did not clearly understand the subject on which he wrote:—

Emerson's Proposition.	*Proposition Criticised.*
"If the matter of any gyrating body were actually to be placed in its centre of gyration, it ought either to be disposed of in the circumference of a circle, whose radius is S O, or else into two points, diametrically opposite, equal and equidistant from S."[28]	"If the matter in any gyrating body were actually to be placed as if in the centre of gyration, it ought either to be disposed in the circumference of a circle whose radius is R, or at two points R, R,′ diametrically opposite, and each at the distance R from the centre." p. 230.

Your reviewer next informs us that centrifugal force is not always "directed towards a fixed centre," in which I perfectly agree with him, and am not aware that any person has stated the contrary.

He then points out an error of Dr. Gregory's, relating to flywheels, which had escaped my observation, and one of my own, in equation (I) page 373, which should read $\frac{1}{4} \mathrm{L P_3} = \mathrm{E}(\delta + \Delta)$; but which fortunately does not affect any other part of the subject. He then states that the total forces of longitudinal compression and tension are equal and opposite; this is, however, only the case when the applied forces are perpendicular to the beam, for when inclined at any angle, Professor Moseley has shown that the *difference* of the forces of compression and tension is equal to the resultant of the applied forces multiplied by the sine of the angle which it makes with the normal to the neutral line at the point of rupture.[29]

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[7] Elementary Treatise on Mechanics, p. 6.
[8] A course of Lectures on Nat. Phil., vol. 2, p. 37.
[9] New Math. and Phil. Dictionary,—Art. "Vis."
[10] Phil. and Math. Dictionary, vol. 2, p. 568.
[11] G. G. Leibnitzii, Opera Omnia, tom. 3, p. 318.
[12] Mechanics of Engineering, p. 132.
[13] Mech. Prin. of Engineering and Arch., p. 133.
[14] Elementary Principles of Carpentry. p. 9.
[15] Elementary Treatise on Mechanics., p. 34.
[16] Mech Prin. of Engineering and Arch., p. 424.
[17] Practical Engineer's Pocket Guide, p. 17.
[18] Principles of Mechanics, p. 66.
[19] On the Free Motion of Points, p. 96.
[20] A Treatise on the Rectilinear Motion and Rotation of Bodies, p. 35.
[21] New Math. and Phil. Dictionary,—Art. "Force."
[22] Phil. and Math. Dictionary, vol. 1, p. 535.
[23] Mechanics of Engineering, p. 133.
[24] A course of Lectures on Nat. Phil., vol. 1, p. 43.
[25] Phil. and Math. Dictionary, vol. 1, p. 297.
[26] Principles of Mechanics, p. 81.
[27] New Math. and Phil. Dictionary,—Art. "Centre of Gyration;" and Encyclopædia Met,—Art. "Mechanics," vol. 3, p. 134.
[28] Principles of Mechanics, p. 83.

In conclusion, if your reviewer is right, it follows that Dr. Hutton, Dr. Young, Professor Barlow, Dr. Whewell, Professor Moseley, Professor Wallace, Emerson, and Tredgold, one and all of them, *must be wrong;* but I think that any reasoning man will require something more, to convince him that the *laboured demonstration* of these men, who have hitherto justly been regarded as high authorities on the subject, are false, than the mere *denial* of an individual writer. Considering, then, with whom it is that your reviewer is at issue,—not with Dr. Gregory and me, but with all the first mechanical and mathematical writers who have lived,—would it not have been wiser, had he assumed a little less positive tone in his attempts to lay down the law?

I remain, &c.,

London, July 15, 1848. HENRY LAW.

* * Mr. Law cannot accuse us of want of good nature, for we print his letter at full length. We had no original prejudice against Dr. Gregory's book. The author has a kind of celebrity from his position at Woolwich, and from having written copiously, which, though he has not made a single step in the advancement of science, led us to imagine him capable of compiling a book like the present without gross and systematic blundering. The first two or three mistakes occasioned a little surprise, but were charitably attributed to inadvertance. It was only slowly and reluctantly that we yielded to the conviction, that the book was radically and essentially erroneous, and that a real mathematician could not by any chance have written it.

Still, we clung to the hope that Mr. Law was guiltless of the various delinquencies to which he had given his editorial sanction. It was, at least, a good-natured excuse—a pious fraud, to delude ourselves and readers with—that he had uttered false coin, *not* well knowing it to be spurious. Alas! even this pleasing self-delusion is destroyed.

The various subjects of discussion are not questions of authority, but of reason. If Newton, Lagrange, and Laplace were to arise from the dead, and assure us that they had discovered the ordinary multiplication table to be incorrect, not even their united testimony would produce conviction in our minds. We may as well set out by avowing, that if those illustrious names were quoted in support of Dr. Hutton and Mr. Law, even they would not produce the slightest change in our convictions. We should feel perfectly certain that their words were misquoted, or strained beyond their intended signification, or—(out it must come) that they had lost their wits!

Our task is a very simple one as regards the definitions; it is to refer Mr. Law to books in which he will find them correctly laid down. That Barlow, Hutton, and Young, have fallen into the same mistakes as Dr. Gregory, only corroborates an opinion independently arrived at—that they were just the men to do so. The distinctions between the cycloid and the trochoid are given correctly, and in exact accordance with our criticism, in Hall's "Differential Calculus, and in page 137 of the "Examples on the Differential Calculus," by the late D. F. Gregory, fellow of Trinity College, Cambridge, one of the most profound analysts in Europe—and therefore a very different mathematician to Dr. Gregory of Woolwich.

Professor Peacock, in his collection of Examples on the Calculus, distinguishes in a similar manner, between the cycloid and trochoid. The other curves in question are thus defined by him, page 192:—

> "If one circle revolve upon another as its base, and in the same plane with it, it is called the *Epitrochoid*, which becomes the *Epicycloid* when the describing point is in the circumference of the revolving circle. If a circle revolve in a similar manner upon the concave part of the circumference of another circle, the curve described by a point in its plane is called the *Hypotrochoid*, which becomes the *Hypocycloid* when that point is in the circumference."

The definition of equilibrium criticised by us, begins "When forces that act upon a body destroy or annihilate each other's operation, so THAT the body remains quiescent" &c. If the words "so that" have the meaning generally adopted by persons who speak and write the English language, it is here asserted that if the forces acting on a body destroy each other's operation, the body must be at rest. It is wearying to have to repeat the correction of so obvious a blunder, but we have again to tell Mr. Law, that the case of uniform motion has been carelessly overlooked by his author. On Dr. Whewell's authority, it is declared that statical forces are called pressures; but he does not deny what we asserted, that dynamical forces are called pressures also; repeated instances of such a use of the expression, may be found in his works.

Dr. Gregory's assertion as to the manner in which Leibnitz used the term *vis viva*, is said to be confirmed by Barlow and Hutton. However, we need not inquire at second-hand what Leibnitz said, or did not say, because his very words are quoted at length. Now, does Mr. Law really mean to assert that in the Latin quotation *vis viva* is called a force? If so, all we can reply is, that he displays considerable fortitude under trying circumstances.

The Latin quotation first specifies the cases in which the two things called *vis mortua* and *vis viva* respectively exist—the former where there is no motion, and the latter where there is motion. Then it is added, that "where a body has been some time falling, or a bow has been some time unbending itself, or in any similar case, there is *vis viva*, generated from the continued infinite impressions of *vis mortua*,"—a perfectly distinct recognition of the truth which we asserted, that *vis viva* is not force, but something generated by it. Of course, the true interpretation of the phrase must be obtained from the context—not from an arbitrary translation of the word *vis*, which has a great diversity of meanings.

Professor Moseley's statement of the principle of *vis viva* is so clear, that it is really marvellous that Mr. Law did not perceive that he quoted an authority decisive against him. He says *vis viva* is a force; Moseley, that it is equal to a certain amount of *work of forces:* "work" being previously explained to be the product of force and distance. It is also important to remark, that *vis viva* is not said *to be* work, but *to be equal* to work. Twenty shillings are equivalent to a sovereign, but they *are* not a sovereign, but differ from it in weight, size, colour, and almost every other particular, except current value.

We will follow the example of printing the contradictory statements side by side. The case then between the authority last quoted and our present correspondent, stands thus:—

Mr. Law.	*Professor Moseley.*
Vis viva of a body "is THE WHOLE mechanical effect which it will produce in being brought to a state of rest."	"The difference between the aggregate work of the accelerating forces of the system and that of the retarding forces is equal to ONE-HALF the *vis viva* accumulated or lost in the system."

The discrepancy between "the whole" in the one quotation, and the "one-half" in the other, would be a fatal objection to Mr. Law's views if no other existed.

The following definitions, in which, be it observed, "force" is not even mentioned, are conclusive as to the use of the phrase *vis viva* among modern mathematicians:—

> "The *vis viva* of a particle is the product of its mass and the square of its velocity."—Earnshaw's Dynamics, page 177.
>
> "Since the publication of D'Alembert's work, the term *vis viva* has been used to signify merely the algebraical product of the mass of a moving body and the square of its velocity."—Walton's Mechanical Problems, page 387.
>
> "The sum of all the bodies of a system each multiplied into the square of its velocity is called the *vis viva* of the system."—Whewell's Elementary Treatise on Mechanics, page 292.
>
> "The term *vis viva* is still used to express the product of the mass and the square of the velocity."—Pratt's Mechanical Philosophy, page 202.
>
> "On appelle *force vive* d'un point matériel, ou, plus généralement, d'un corps dont tous les points ont les meme vitesse, le produit de son masse par le carré de cette vitesse."—Poisson Traité de Mécanique, tom. ii., page 29.

The proposition respecting tie-beams was condemned by us because some of the forces acting on the beam and strut are neglected; and we showed as corroboratory proof of the incorrectness of the solution, that it led to an absurdity. Of all this our correspondent takes no notice; but refers us to Professors Moseley and Whewell. If both these references were relevant, which they certainly are not, they would not justify a palpable and obvious error; and we must tell Mr. Law plainly, that we should have had far more respect for him as an opponent, if he had made the necessary correction, instead of endeavouring to transfer the blame to authorities no way involved in it. As for Professor Wallace's adoption of the problem, we can only say that in this instance he has made an unfortunate selection.

By inserting the word "uniform," in quoting our remarks upon page 193 of the "Mathematics for Practical Men," Mr. Law makes us talk nonsense. However, we freely admit that we here inadvertently mistook the purport of one of Dr. Gregory's sentences, and though what we said was true, omitting the word which Mr. Law forces upon us, Dr. Gregory's sentence *with* that word is perfectly correct also. Mr. Law unnecessarily injures his case by a quotation from Dr. Whewell, which has not the remotest connection with the subject. Dr. Whewell speaks of accelerated velocities in the several bodies: Emerson and Gregory, of uniform velocities only. The law stated by the two latter depends for its proof on wholly different principles to that enunciated by the former. The connection between them which Mr. Law attempts to establish, is of that kind which exists between the 1st of March and the foot of London-bridge. We have not the slightest objection to let the whole dispute between us and our correspondent rest on the reply of *any* real mathematician to this question:—Does not this attempt to confound two principles essentially different display either the most profound ignorance or the most hopeless confusion of thought respecting the science of exact mechanics? If Mr. Law can get *one* competent umpire to answer that question in his favour, we will give up the whole controversy.

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[29] Mech. Prin. of Engineering and Arch., p. 495.

The idea respecting the *natural slope* has the merit of originality. Mr. Law "*prefers*" giving it a meaning which it has not hitherto received. He says, the natural slope is the slope along which rupture would take place if the revetement wall were removed. Now, the definition of natural slope, as ordinarily used, is that it is the *very slope* along which rupture cannot take place—its friction being just capable of sustaining the superincumbent mass *without* the assistance of the wall.

We are next said to have misunderstood Dr. Gregory respecting the conditions on which he discusses one of the cases of the equilibrium of arches. This is mere assertion. We repeat the counter-assertion, that in the case referred to, conditions are assumed which, quite independently of all statical considerations, render it impossible that the arch from its mere *form* could be overturned. Mr. Law says that "no such conditions are assumed, or indeed necessary." Not necessary, indeed! Why, they make Gregory's lucubrations nonsense. Does Mr. Law mean to infer, that *sometimes* it is necessary that Gregory should talk nonsense?

Then follows an accusation against us, of having maliciously misquoted and perverted the words of the book under review. The best answer to personality is silence.

With regard to the "careful and often-repeated experiments" which Mr. Law asserts to have been repeatedly made, we have already explained, as clearly as we could, the confusion here made between mechanical experiment and pure geometrical measurement of distances. The words, "uniformly retarding force," involve the very conclusion which is declared to be the result of numerous trials; just in the same way that the first proposition in Euclid is a pure deduction from his definitions. *Experiment* would be as preposterous in the one case as the other.

The following quotations, the former from Moseley's Principles of Engineering, the latter translated literally from Poisson's Traité de Mécanique, are offered in a faint hope of clearing Mr. Law's ideas respecting the resistance of inertia:—

> "So many difficulties, however, oppose themselves to the introduction of the term *vis inertiæ*, associated with the definitive idea of an opposing force, into the discussion of questions of mechanics, that it has appeared to the author of this work desirable to avoid it."—Principles of Engineering, page viii.
>
> "It is important to rectify an inexact expression, which is often employed and tends to a confusion of ideas. Imagine that a body is placed on a horizontal plane, and that it is not retained by any friction. If I wish to make it slide on this plane, it is nevertheless necessary, on account of the inertia of the matter, that I exert some effort; if to this body be added a second, then a third, &c., it is necessary that I employ, to produce the same degree of motion, a force more and more considerable. I shall in each case experience a sensation of the effort which I shall be obliged to exert; but I must not thence conclude that the matter opposes any resistance to this effort, and that there exists in the bodies what is very improperly termed *resistance of inertia.* When any one expresses himself in this manner, he confounds the sensation which he has experienced, and which results from the effort which he has made, with the sensation of a resistance which does not exist."—Traité de Mécanique, No. 120.

Respecting the problem of the weight oscillating at the extremity of a flexible string, Mr. Law himself shall be umpire, if he will promise to make the following experiment:—Let the oscillating weight descend a vertical distance of one foot, and let the peg interfere with the string at a distance of one inch from the weight. If, then, he find the string so accommodating as to stretch itself out the odd eleven inches, necessary to permit the weight to rise to the former height, we will acknowledge ourselves beaten.

The propositions printed side by side after the next paragraph, amount to this: If a body be in one place, quoth Emerson, it ought to be somewhere else. To which Dr. Gregory responds, Amen.

From the manner in which Mr. Law meets our criticism upon his author's erroneous definition of centrifugal forces, we infer that he finds there is no tenable defence. He also candidly acknowledges two other mistakes, and therefore we have nothing further to say of them. Our remark on the equality of the total forces of compression and tension in a beam, were manifestly restricted to the particular case under examination—that where there are not appreciable horizontal forces acting externally on the beam.

It is important to observe, that throughout Mr. Law's letter, he never discusses any question on *its own merits.* He contents himself with appealing to authorities. This course is at least dexterous, for with the unlearned an array of imposing names is but too apt to carry conviction. The authorities quoted are, however, easily disposed of: several of them are of little weight when opposed, as we have shown them in several instances to be, to the most profound continental writers. The rest of the citations are either irrelevant, or make directly against Mr. Law's tenets. Had he suffered the controversy to rest on its own abstract merits, our task would have been more easy to ourselves, and more satisfactory to the cause of truth. It would be an insult to his understanding, to suppose that he did not perceive that many of the errors which he defends by quotations were, in reality, indefensible in any other way. Casuistry and perverse ingenuity, however well suited for mere disputation, are never the weapons of a man of science.

Mr. Law seems very fond of appealing to Woolwich Professors. Why did he not quote Mr. Davies or Mr. Rutherford—gentlemen who have acquired for themselves reputations not confined to the regions of Woolwich? It is very unfortunate, that the only two works written by Woolwich mathematicians which we have lately had to review, have been anything but very creditable to the scientific character of that institution; and this is the more to be regretted, since, as we have already hinted, inaccuracy in conceiving physical ideas, and clumsiness in developing them analytically, are not common to all the Professors—at least to all the *Teachers*—of the Royal Academy.

SANITARY LAWS AT HOME AND ABROAD.

The most destructive scourges of the human race are the epidemics and contagious diseases produced by a polluted atmosphere, and the congregation of men in crowded cities. Famine and the sword slay their thousands—the pestilence that walketh at noonday and in darkness its tens of thousands. The ravages of war and want are, partially at least, within human control; but when the destroying angel comes, borne on the breath of "quick pestilence," human skill and energy are all but powerless against him This sudden destruction, moreover, is not all that is caused by the infected air and artificial habits of populous places. The maladies indirectly induced, the remote consequences of a morbid habit of body which renders it a kindly soil for the future seeds of death, the degeneracy of sickly offspring who reap in a later generation the bitter fruit sown by their parents—these are among the penalties which the denizens of large towns too often pay for inhaling the hot vapours of the foundry or furnace, or breathing the stagnant air of crowded courts and lanes, where overhanging house tops shut out the pure breath of heaven.

Prevention is almost the sole defence against these evils; for when once developed, they are either too sudden or too deeply rooted to admit of effectual remedy. A curious chapter in the history of the internal economy of states is that which the various sanitary provisions adopted under different forms of government. Nations working for the same end, the public health, seek it by entirely different means. On the Continent, where the rights and liberties of individuals seldom constitute a serious obstacle to State purposes, the most stringent sanitary regulations have long existed and the surveillance of police, which is almost unknown in England, constitutes the principal means of effecting them. Here, the public jealousy of state interference, and a sensative regard for the rights of property, have long prevented the institution of a general organised sanitary system. The Public Health Act, which will soon be the law of the land, makes the first provision for such a system in England; and it becomes, therefore, interesting to compare it with the machinery adopted for the same purpose by our continental neighbours.

The Public Health Act comprises two parts, the construction of a new *Central Board* of Health to put it in motion, and of *Local Boards*, for the purpose of carrying its details into effect. As the functions of these former and latter are entirely distinct, they may be stated separately.

The Central Board of Health consists of three commissioners, and the President is the first Commissioner of the Woods and Forests for the time being. These commissioners, upon the petition of one-tenth of the rated inhabitants of any town, may direct an examination as to its sewerage, drainage, supply of water, &c.; and after hearing the representations of parties locally interested, may report to her Majesty. *An order in Privy Council* may then be made, directing the application of the Act in cases where existing local Acts are not infringed upon, and the local boundaries are intended to remain unaltered. This order simply directs that the Act, or any part thereof, is to be put in operation.

Where, however, it appears necessary to alter the constituted local boundaries for the purposes of the Act, or to apply it in cases where local improvement acts already exist, the order in council is not made; but the General Board of Health have power to make such a *provisional order* for the application and execution of the Act as they may deem fit. This provisional order cannot be carried into effect until it have been sanctioned by Parliament. The orders issuing either from the Privy Council, or from the General Board of Health may, from time to time, be amended or extended, after due notice to persons locally interested.

The only other functions of the General Board are to determine appeals from certain decisions of local general boards, and to regulate intra-mural interments, by certifying upon the representation of the local boards, that certain places of interment are dangerous to health, and appointing a time after which it ceases to be lawful to bury in such places.

The Local Boards have much more diversified duties. When the whole of a district formed under this act is within a corporate borough, the corporation constitutes the local board. In other cases, it is selected by rate-payers. The offices assigned to it are principally these—to appoint inspectors of nuisances and officers of health—to prepare maps exhibiting a system of sewerage—to assume the control of public sewers, and purchase private sewers—to alter, extend, and cleanse the same—to cleanse and water public streets—to provide public necessaries—to register or provide and regulate slaughter-houses—to prevent (subject to an appeal to the general board) the establishment of noxious manufactures, and the erection of churches, hospitals, factories, or any other large building, without proper means of ventilation—to register, and if necessary, cleanse, or disinfect common lodging-houses—to restrict the use of under-ground cellars as dwelling-rooms—to level and pave streets—to move water or gas pipes, provided that their use and efficiency be unaffected—to prevent (subject to appeal to the General Board) the laying out new streets of objectionable width and level—to purchase premises for the purpose of widening streets—to provide public walks and pleasure-grounds—to construct water-works—to compel occupiers of houses to receive a proper supply of water—to supply cisterns and conduits for gratuitous use—and to levy rates for the purposes of this act. In cases of damage by the acts of the Local Board, compensation is to be made; and if its amount be disputed, it is to be settled by arbitration. The bye laws of the local boards are to be submitted to the Secretary of State for the Home Department for his approval.

It will be seen that in the actual administration of the act, little or nothing is assigned to the Central Board. The power is almost entirely in the hands of the Local Board, which is not amenable to the General Board for acts of omission or commission, except in one or two instances above referred to, in which appeal may be made. And as the Local Boards are popularly selected, the power of putting the act in force ultimately belongs to the great body of inhabitants of the districts affected. There seems, therefore, little reason to fear that people will be compelled to be clean, and drink wholesome water, and breathe fresh air, against their own free-will and consent.

On the Continent, however, the popular voice is not heard on these questions; and the power of enforcing measures for public health, is vested almost exclusively in central or government boards. It must be acknowledged that, notwithstanding the greater stringency of sanitary regulations with France and Germany, the practical effects of them have been even more imperfect than in England. In Paris and Vienna, the sewerage, drainage, paving, lighting and cleansing of streets, are far less complete than in London. On the other hand, our neighbours have local advantages wanting here. The boulevards, and other public walks of Paris, are far more extensive in relation to the number of the population, than the parks of London; and are also more easily accessible on account of the less size of the former city. There are no cattle markets within Paris, and the slaughter-houses are removed to the suburbs. Vitriol manufactories, and similar abominations, are not suffered to pour their pestilential vapours into the very heart of the city, nor consume human life and vegetation as they do in the close vicinity of this metropolis. Intramural burials have long been prohibited in Paris, and are of very rare occurrence.

The sanitary state of the capitals of France, Austria, and England, is partially indicated by the rate of annual mortality. It appears from the returns of the Registrar-General, published in 1845, that the average annual mortality for every hundred inhabitants of

Vienna, is about	4·898 per cent.
Paris (department of the Seine) ...	3·038
London	2·392

But, of course, the rate of mortality is influenced by many other considerations besides sanitary regulations; the vicissitudes of climate and the habits of the people having important effects on the duration of life.

It is comparatively recently, that Paris assumed the appearance of a well-lighted and well-paved city. The idea of paving it is said by Fœderé, a copious writer on the hygiene of France, to have originated with Philip Augustus, who, looking out of his palace windows one rainy morning, and surveying the marshes in which his capital was built, conceived the brilliant idea that it would be a convenience to himself and subjects to walk upon dry ground. So early as 1486 an ordonnance of the *Prevot* of Paris prohibits the erection of noxious and offensive manufactories within the town; and the records of French municipal law refer to many subsequent regulations of a similar kind. No very effectual or important measures seem, however, to have been taken till after the great revolution; and to Napoleon, the most uncompromising of reformers, is due the credit of founding the present system of law regarding public health. His attention having been directed to the insufficiency of the existing laws on the subject, the Academy of Sciences was consulted, and the class of physical and mathematical science reported to the Government, on the effects of various manufactures on the health of the people. On this report was founded an imperial decree of 1810, subsequently confirmed by a royal ordonnance of 1815, which, with some modifications suggested by subsequent experience, constitutes the modern sanitary code of Paris. For an account of it we have consulted, amongst other works, that of Fœderé, last cited; *Trebuchet's Code Administratif des Establissemens dangereuses et insalubres* (8vo., Paris, 1832); *Parent Duchatelet's Prostitution dans le Ville de Paris* (8vo., Paris, 1836); and the *Hygiène Publique*, of the same author.

The principal feature of the decree of 1810 was the division of noxious and offensive trades and manufactures into three classes, the first consisting of those so prejudicial to life and vegetation as to be required to be moved to a distance from human habitations; the second and third of those noxious in the less degrees. The distinctions between these three classes are carefully defined, and the exactness of the definition is practically of great importance, as the mode of applying for licenses for their establishment are different for each. The formalities necessary previous to the erection of a manufacture of the first or most dangerous class are very numerous and stringent. After notice publicly advertised of the intended application for license, the Mayor of the commune (or in Paris the Commissary of Police), reports on the nature of the localities infected, the distance of habitations, and the character of the processes to be employed. This investigation is technically termed the inquiry *de commodo et incommodo.* His report is referred to the *Conseil de Salubrité*, a body which takes cognizance of the establishment of all manufactures, which are "classified," or included in either of the classes above-mentioned. The *Conseil de Salubrité* delegates a sub-commission of its own members, to examine the locality of the proposed manufacture, in company with the Mayor, and to report upon the nature and importance of the manufacture, its salubrity, or inconvenience, the rate of flow of water required in its processes, the sufficiency of the apparatus, the merits of the principles on which it is constructed and applied, and lastly, on the admissibility of the application, and the conditions on which license should be granted. This report of the sub-commission is presented to, and discussed by, the general board. Among the other functions of the *Conseil de Salubrité* included the inspection of barracks, prisons, markets, and other public buildings. Besides the *Conseil de Salubrité* at Paris there are similar bodies at Marseilles, Lille, Bordeaux. and other large towns.

The *Architect-Commissary* is another public officer who discharges most important and valuable duties. A detailed *plan* of the proposed manufactory is submitted to him before the commencement of the building, and on its completion, he inspects the details of the workshops, furnaces, chimneys, and other parts, and determines whether the laws and prescribed conditions have been strictly observed in their construction. He must also also assure himself, as far as possible, of the stability and security of the buildings, both internally and externally, and ascertain whether the arrangements and situation agree with those indicated upon the plan previously deposited with him. M. Trebuchet, who speaks from practical experience as chief of the office in the Prefecture of Police which takes cognizance of establishments affecting public health, remarks, that this verification by the plan would be greatly facilitated if all plans were required to be laid down to the same scale; for instance, five millimetres to the metre. The Architect-Commissary has another very important duty. Whenever it becomes necessary to classify an altogether new manufactory, he is consulted as to the class in which it is to be placed. Upon all complaints against existing manufactories, his opinion is also required.

The actual execution of the laws respecting manufactories is assigned to the Mayor of the commune. His vigilance must be exercised to see that they are never infringed or evaded; and it is his especial office to maintain a surveillance, so that no manufactory be established without the requisite license.

To the Prefecture of Police the supreme municipal power belongs. The Prefecture institutes periodical inspections of authorised manufactures, and on its authority the execution of all official acts depends.

Steam-engines are put under a peculiar system of regulation as the ordinary laws respecting classified manufactures are not entirely applicable to them. As a general rule, stationary engines are ranked in the third class of manufactories. The rules for high-pressure steam-engines have been so greatly altered from time to time, that it is scarcely worth while to refer to more than one or two of the most important. All high-pressure engines are, or used to be, tested hydrostatically to triple their intended working pressure which is then marked upon them by a government stamp. Every new steam-boat is inspected by a commission of engineers appointed by the prefecture of police, and is required to have on board a person duly qualified to superintend the machinery, and see that it is in proper working order. Captains of steam-vessels are compelled to register the limit of their numbers of passengers, and are made personally liable for accidents arising from overcrowding or excessive speed.

The sewerage of Paris has been made the subject of that scientific and systematic inquiry which characterises the public administration in France of all matters relating to the acts. It appears from the *Hygiène Publique* of Parent Duchatelet, published in 1836, that the total length of sewers in Paris, was

Before 1830	...	...	43,340 yards.
From 1830 to 1834		...	23,790
In 1835	...	...	5,460
In 1836	...	...	4,040
			76,630

Some of these sewers are of great size, and have been excavated at great expense. That of the Rue Rivoli was excavated at an expense of £32,000, and with a solidity and excellence of workmanship which appears extravagant. Another of the largest sewers, that under the Rues St. Denis and Du Poncean, contains the pipes of pure water which supply the fountain of the Place des Innocens. Napoleon took great interest in this work, on account of the novelty of the application of the sewer to the purpose of conveying water, and examined it in all its details.

Paris stands on a gravelly alluvial soil; and the level above the bed of the Seine, is so small as to render inundations of frequent occurrence. This circumstance increases the difficulties of emptying the sewers of which the river is the general receptacle. The impetuosity of the current during winter has, however, the effect of preventing a permanent accumulation of impurities in its channel.

In supplying Paris with water, the conduits from the great aqueduct pass through the sewers. The practicability of rendering the sewerage serviceable for the purposes of agriculture, is recommended in a memoir presented to the prefect of police in 1835, by a sub-commission of the *Conseil de Salubrité* It appears from the same authority, that the quantity of fœcal matter deposited in the Seine, is so small as to have no appreciable influence on the purity of its waters; and in a report of the *Academy of Sciences*, it is stated that the volume of water supplied by the flowing of the Seine, in a given time, is 9,600 times that of the utmost amount of impure substances which can be deposited in it during the e period.

In Austria the sanitary institutions of the whole empire are placed under one general system, which appears to be carried out in an effectual manner. The laws relating to public health belong to the best part of the imperial code; and there are few countries in the world of which the medical institutions are conducted on such a magnificent scale. The administration of them is entrusted by the Austrian Government to a duly qualified corps of officers, at the head of whom, in every province, is a chief officer, called the *Proto-Medicus;* and in every circuit there are sub-officers, who constitute boards of health for their own districts. The functions of the *Proto-Medicus* are to inspect medical institutions, hospitals, lunatic asylums, and prisons, to regulate the medical police, and report on epidemics. The poor-law system of Austria provides for the gratuitous supply of medicine to the poor, and the attendance upon them by the district medical officers.

The Austrian police inspect all food exposed for sale, and have the power of examining houses and lodgings to ascertain that they are in good and healthy condition. All burial grounds are required to be at a distance from towns, and the instances in which burial in family vaults is permitted are exceedingly rare.

The vicinity of Turkey renders the quarantine laws of Austria very strict. A military cordon of 4,200 men is always maintained on the Turkish frontier, and the number can be increased to ten thousand on occasions of danger. Owing to the vigilance thus exercised, it is asserted that the plague has not crossed the frontier for upwards of a century.

On the whole review of the Continental and English sanitary systems, the essential distinctions between them appear to be that the former are administered by central, the latter by real, government; the former makes public health a matter of state policy, the latter of popular discretion. Our own system has the advantage of pleasing the governing power in the hands of those whose personal observation renders them the best judges of local wants, and generally the most zealous protectors of local interests. The question whether they will prove also the most strenuous promoters of sanitary improvements, must be determined by experience alone. In numerous cases, the intelligence of the beople will dictate, that public health be regarded as the paramount object. Other local bodies, from motives of false economy, may refuse to tax themselves for providing the means of health, or from apathy, may decline to exercise vigorously the powers of the most confided to them. Yet, these cases will be exceptioned; for the most prejudiced and the most indifferent must learn, sooner or later, that sordid neglect is never cheap. Health is wealth; for health gives energy and hope; and these beget industry and frugality, which are the sources of all wealth. But squalor and disorder are life—consuming, and the very canker-worms of social existence: they beget sloth and indifference—and these are spendthrift: they beget crime and disease—and these destroy.

WATERLOO EXTENSION OF THE LONDON AND SOUTH-WESTERN RAILWAY.

The extension of the London and South-Western Railway from Nine-Elms to Waterloo-bridge-road, was opened on the 11th ultimo. It appears to us that it would have been far better if the Waterloo Station had been made on the vacant ground adjoining, north of the present Waterloo terminus, and the principal entrance in York-road. The entrance to the railway would then have been as near to Westminster and Hungerford-bridges as it is now to Waterloo-bridge, without increasing the distance to the latter place, or the length of the railway. This alteration might now be easily made; it would save nearly half-a-mile, and eight minutes' walk, to foot passengers from Westminster and Charing-cross; the present approaches might be retained for a goods depôt—and if a steam-boat pier were made adjoining to the Surrey approach of Hungerford-bridge, and arrangements made with the steam boats to come direct from London-bridge to the pier, the extension of the railway to London-bridge might be abandoned, and thereby nearly a million sterling saved. This would, we are sure, enhance the value of the shares, and give confidence to those capitalists in the city who are now alarmed at the apparent reckless manner in which funds are being expended by railways on branches and extensions.

"The works of this undertaking were commenced in July 1846, by Messrs. Lee, the contractors, who engaged to complete the works by the 1st July, 1848. Mr. Thompson acted as the superintenden to the contractors, and Mr. Curlieu on behalf of Mr. Locke, the engineer. The length of the new line is nearly 2¼ miles. The first quarter of a mile is carried over an embankment; then succeeds a viaduct, consisting of six massive iron girder bridges, and 300 arches (exclusive of those forming the present station in the Waterloo-road). These arches, which are expected to form a very considerable item in the receipts of the company, have been so carefully constructed, as to be easily applicable to various purposes, and their perpetual dryness has been insured by the application of the Seyssel asphalte, which has rendered them impervious to wet. There are four distinct lines of rail, and the quantity of iron alone consumed in laying down what is technically called the 'metals,' is at least 1200 tons, independently of about 800 tons weight consumed in the erection of the bridges. In the construction of the viaduct and station of the Waterloo-road, upwards of 80,000,000 of bricks have been consumed; and the present terminus, which is all on arches, covers a space of three-quarters of an acre of ground, its width being 260 feet. The major part of the present terminus has been coated with Claridge's asphalte, so that the arches on which it rests may with safety be made use of as storehouses, &c. To the present terminus in the Waterloo-road there are no less than four approaches for carriages and foot-passengers, the pedestrians having in each approach footpaths 8 feet in width. The stations at both Waterloo-road and Vauxhall are only temporary. The fares on the main line are increased as follows:—First class, 6d.; second class, 4d.; third class, 2d. The Nine Elms station is now closed entirely to passenger traffic."

STEAM-ENGINE GOVERNORS.

We give the following extract of the *Daily News*, from the police reports of the Mansion-House of the 22nd ultimo, as some allusion is made to our *Journal*, and to state that we were much surprised, after the exposure of the letters therein given, that Mr. Cousens should have had the audaciousness to send us the paper for publication. Immediately after its receipt we returned it to the author, and ordered to be cancelled an introductory paper on Steam-Engine Governors, which Mr. Cousens sent us previously to the appearance of the police report. We trust that Mr. Woods' praiseworthy resolution may deter others from attempting a similar proceeding. It is fortunate for Mr. Cousens that he had such a lenient magistrate to hear the case.

Mansion-House, July 22, 1848.—Alleged Attempted Extortion of Money.—Mr. R. B. Cousens, of No. 4, Bedford-place, Old Kent-road, appeared to answer a charge of having offered to prevent the printing and publishing of certain matters touching the complainant with intent to extort money. The charge was brought upon the 6 and 7 Vic., cap. 96, sec. 3, and it was stated, for the prosecution, that the defendant had by means of the following letter endeavoured to accomplish his purpose:—

"4, Bedford-place, Old Kent-road, July 8, 1848.—Sir,—I have written for publication a small treatise on the cause of the inefficiency of steam-engine governors; and in investigating the principles of the chronometric governor, of which you are the patentee, I find myself compelled to speak of it in a way which may, perhaps, lessen its value as a commercial speculation. Now, as I write only for pecuniary profit, I am willing to withhold the paper from the public eye, if, after perusing the accompanying copy of that part of it which concerns the chronometric governor, you feel disposed to purchase it at a fair remuneration. It is quite immaterial to me whether it be read by many or by one, whether it be preserved in the pages of a scientific periodical or destroyed, if I profit by it. It is unnecessary to say more in explanation of my object in transmitting to you the MS. copy. I shall merely add in conclusion, that after Wednesday, the 12th instant, I shall feel at liberty to forward the original to the publisher, unless I am previously favoured with some communication inducing me to withhold it.—Robert B. Cousens.—To Joseph Woods, Esq."

Upon the receipt of that communication Mr. Woods sent to the writer a note, to which the following answer was returned by that gentleman:—

"Bedford-place, July 10.—Messrs. J. Woods and Co.—Gentlemen,—In obedience to your request I have to acquaint you that my paper on 'governors,' if published in one of the periodicals, will most likely appear in the 'Civil Engineer and Architect's Journal;' but that I am yet undecided as to whether it shall appear in a periodical or form part of a separate pamphlet on that and another part of the steam-engine. In all probability I should pursue the latter course, as being the more remunerative.—Robert B. Cousens."

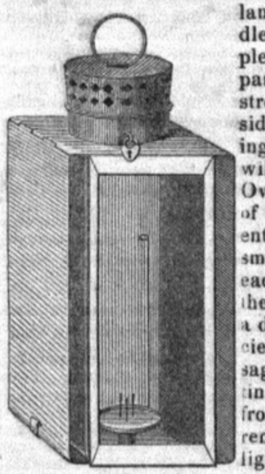
After he had received the second letter Mr. Woods resolved not to submit to be victimised, and, having it in his power to produce witnesses in support of the charge, was dvised to represent the case at the Mansion-House.—It was now urged for the defence that the case was one in which the magistrate had no jurisdiction, as the act of parliament directed that, in case of a libel, the person accused should be proceeded against by indictment. The offer made could not, it was said, be construed into an offer to refrain from publishing a libel upon the patentee.—Alderman Gibbs said it was undeniable that the letter was written for the purpose of raising money.—Mr. Humphreys contended against Mr. Hobler's view of the case that the magistrate had the power to compel the defendant to appear to answer an indictment as upon a charge of felony or misdemeanor of an ordinary description made before him. The charge against the defendant was clearly misdemeanor, as declared by the statute, and indictable at the sessions, and he submitted that the magistrate had full jurisdiction as to holding the defendant to bail.—Mr. Hobler said if Mr. Woods considered that an offence had been committed he had his remedy. He could go to the grand jury with his indictment, and if they found a true bill, he could bring the matter to trial; and if the defendant was found guilty, punishment would be inflicted; but it would be a very difficult matter to show his client's liability under such circumstances.—Mr. Humphreys: The charge against him is that he has attempted to extort money by means of threats.—Mr. Hobler said that if Mr. Cousens used threats, those threats were by no means conveyed in language of which cognizance could be taken.—Alderman Gibbs said he could not see that the letter was a libel upon Mr. Woods, or that it contained matter of which he could take any notice, whatever tendency and intention it might have had to extort money. He should therefore dismiss the case.

NOTES OF THE MONTH.

The Athenæum and the Engineers.—The *Athenæum* of the 15th ult. contains some remarks in reply to our's of last month, with regard to Mr. Chadwick, the Military Engineers, and the Metropolitan Survey. The *Athenæum* has replied to our remarks, as if we advocated the employment of the Surveyor's Association; whereas, we did not advocate the employment of that association, or of any particular individual or individuals. We objected to the employment of the military engineers, and named several civil engineers, who were quite competent "to conduct a trigonometrical survey, which involves the nicest points of astronomy, and requires all the resources of mathematical analysis." We again refer to the minutes of proceedings of the Institution of Civil Engineers, in the columns of our contemporary, as giving evidence of the attainments of the members in all respects, as exemplified in the discussions on atmospheric resistances, the atmospheric railway system, and the many mathematical debates which have occupied the Institution of late years. We now refer our contemporary to the account of the Ordnance survey of Liverpool, given in a report lately issued by the borough engineer, and which we shall publish next month.

South Hackney Church.—This has been three years under construction by Mr. Hakewill, and was consecrated on the 20th of July. It is in the Early English style, and in the shape of a cross, and is executed with great care and solidity. The cost was £15,700, besides land. The length inside is 172 feet; outside, 192 feet; width across the nave and aisles 61 feet, and transepts, 92 feet; height inside to roof, 60 feet, outside to top of spire, 187 feet. There are eight bells, and several painted windows, and a large sum was laid out in decoration. This is a building which possesses considerable merit; for too much has not been attempted, and a good effect has been produced. The proportions are well kept, and an air of grandeur and chasteness preserved.

Independent Chapels.—We are glad to see that such an advance is being made by the Independents in ecclesiastical architecture. Mr. Edward Walters, an architect of ability at Manchester, has been employed in erecting a chapel in Cavendish-street, in that city. It is in the mediæval style, with a tower and spire rising to the total height of 171 feet from the ground, and cost £24,000; so that it will be seen that it is an important building, as it is one likewise highly ornamental and artistic. Another chapel, erected by Mr. Walters, at Darwen, cost £5,000. A peculiarity in this building is a kind of screen raised above the roof; which, though of beautiful design, having no idea of usefulness attached to it, conveys to the mind an impression of superfluity.



Safety Mining Lanthorn.—Mr. Crane, of Birmingham, has forwarded to the *Mining Journal* the following description of a mining lanthorn that he has invented. The annexed drawing is a representation of the safety-lanthorn:—It is adapted to burn composition candles that require no snuffing. The same principles can, however, be applied to oil lamps, if any party prefer oil to candles. The front is made of strong glass; the back of polished tin—the two sides of wire gauze, soldered to the framing, having 900 apertures in a square inch of surface. It will do coarser: but the size stated is safest. Over the wire gauze sides are fixed covers of tin, hinged to the top of the lanthorn, which entirely cover the sides, and are kept fast by a small hasp at the bottom. The lower edge of each tin coverside is bent inwards to rest against the framing—so that the tin plate may be kept at a distance of ¾ inch from the wire gauze. Sufficient space is thus provided to allow of the passage of air for the supply of the light.—These tin coversides are useful to protect the wire gauze from injury and dirt, as well as to stop any current, or "blower," of gas from blowing out the light. No direct current of wind can have any effect upon the light, because there is no admission into the lanthorn but obliquely at each corner. The candle is held between four short wires, soldered in the dish of a moveable socket, which fits into a socket soldered to the bottom; this candle socket is useful for retaining any waste fat that may run down; it can be lifted out by the wire handle, and cleaned, when necessary. The inside of the lanthorn is thus kept quite clean. The candle is put in through the neck on the top of the lanthorn, upon which a hinged lid fits down tightly. The lid is pierced with two rows of holes, through which the heated air and smoke escape; and to the top is fixed a large ring, by which the lanthorn is carried and hung up. This ring is kept cool by a simple, but effective, contrivance. A piece of tin, bent into the form of an inverted cone, is soldered inside the lid, which causes the hot ascending air to flow towards the sides, where it immediately escapes through the openings. To prevent any inflammable gas entering through the lid, a circular disc of wire gauze is soldered inside the rim of the lid—so that no gas can enter but through the wire gauze; this wire gauze will never become red-hot, so that no explosion can possibly occur. The size of lanthorns made is about 5 inches square, and 12 inches high; other parts in proportion. The inside of the tin coversides, and the outside of the lanthorn, are japanned of any dark colour.

The Soap Plant.—In California this plant is used by the people for washing every description of clothing in cold running water. In using it as soap, the women cut the roots from the bulbs, and rub them on the clothes, when a rich and strong lather is formed, which cleanses most thoroughly. To propagate the plant the bulbs are set in a rich soil, and grow luxuriantly in the soft bottoms of valleys or bordering running streams.

Copper Sheathing.—A correspondent of the *Mining Journal* states, as the consequence of his experience, that, in the treatment of the sulphurets of copper, there should be one calcining, one roasting, one smelting, and one refining—four operations in all; and that care be taken that no iron tools be used, except the ladles for the refining process. The carbonates of copper require only two operations—smelting and refining; but if copper pyrites be mixed with the carbonate, it will require three operations instead of two. By attention to these operations all foreign matter will be disengaged. The production of good malleable and pure copper depends on the refiner; the copper is brittle before, and should be stirred with a wooden rod. It requires considerable care to keep the metal to a proper heat until the moulding is finished, to give it due ductility, and make it suitable for the demands of commerce. In general, most operators go too far in the refinery, which renders the metal fibrous, and the result is serious lamination on one side of the sheet. If the copper ore is properly treated in the above operations, this metal is decidedly the best for ships' bottoms. The per centage of copper is also much increased by careful treatment, and the scoria comes out cleaner.

Method of Welding Iron, Steel, and Sheet-Iron.—In an earthen vessel melt borax, and add to it $\frac{1}{10}$th of sal-ammoniac. When these ingredients are properly fused and mixed, pour them out upon an iron plate, and let them cool. There is thus obtained a glassy matter, to which is to be added an equal quantity of quicklime. The iron and steel which are to be soldered, are first heated to redness; then this compound, first reduced to powder, is laid upon them—the composition melts and runs like sealing-wax; the pieces are then replaced in the fire, taking care to heat them at a temperature far below that usually employed in welding: they are then withdrawn and hammered, and the surfaces will be found to be thus perfectly united. The author asserts that this process, which may be applied to welding sheet-iron tubes, never fails.—*Rec. de la Soc. Polytech.*

Dotting Electric Telegraph.—A patent has been recently granted in this country to M. Dujardin, of Lille, for a new kind of electric telegraph in which the signals consist of dots made on paper. The telegraphic pen is fixed to a magnet, and it marks dots on a revolving traversing cylinder. The dots, by a previous alphabetic arrangement, are made to signify letters, each letter being characterised by a certain group of dots. The process is complicated, and must necessarily be a slow one; nor does the inventor, who is a physician, seem to have been aware of the invention of Mr. Morse, which accomplishes the same object more efficaciously, and with a much less complicated mechanism; Mr. Morse's alphabet consisting of short and long strokes, by which means the letters of the alphabet may be indicated by a smaller number of marks than by dots of the same size.

Time Signals.—Mr. Torrop, of Edinburgh, has patented an apparatus for giving notice of the approaching departure of railway trains, so as to supersede the use of bells or whistles. The apparatus consists of a hollow pole erected vertically at any convenient part of the station where it may be most advantageously seen both by those at the station itself, as also by those hastening towards the station. Upon the outside of this pole is placed a large ball, the pole passing through a hole through the centre of the ball, sufficiently large to allow free motion up and down the pole. The ballis suspended by cords, attached at the bottom of the pole to a clockwork movement, having a pendulum, the vibrations of which regulate the descent of the ball upon the pole. When the spring of the clockwork is wound up, the ball is raised to the top of the pole, which then begins to descend to the bottom of the pole, being regulated in its velocity by the clockwork and pendulum. The time of its descent will therefore be regularly the same each time the ball is raised to the top and then allowed to descend. The ball being raised to the top of the pole at the adjusted period of time (say ten minutes before the departure of each train), and then allowed to descend, the position of the ball upon the pole during its descent will give notice of the length of time to elapse before the departure of the train. During the night a lamp is to take the place of the ball.

A few Degrees of Difference.—In an action recently tried in the Court of Queen's Bench, the question was raised whether the variation of three degrees in the inclination of a wood pavement constituted an infringement of the patent right. The defendants (Esdaile and Co.) were licensees under Rankin's patent for wood pavement, and the plaintiffs (Hulse and Co.), entered into a covenant with them to pay certain royalties if they infringed upon the principle of Parkyn's patent. The principle was the inclination of the fibre of the wood to the horizon; and that was described to be from 40 degrees to 70 degrees. The defendants laid down pavement in Cornhill and at a bridge at Chalk-farm; and the blocks they laid down were at an inclination of 73 degrees. The contention for the plaintiffs was, that although the inclination was not within the precise words used in the deed, still that for all practical purposes there was not the slightest difference between 73 degrees and 70 degrees.—Mr. Justice Wightman, in summing up, observed, that if parties chose to bargain in specific terms they must abide by their bargain, and though in practice the two inclinations mentioned could make no difference, still the terms of the covenant were express; therefore, unless the inclination adopted by the defendants was within that limit, the plaintiffs on that ground were not entitled to recover. The jury therefore gave a verdict for the defendant, the plaintiffs having liberty to move to enter a verdict of 50*l.* 1*s.*

College for Civil Engineers.—On Tuesday the 18th ult., the yearly examination of the students of the College for Civil Engineers was held at Putney, and was attended by H.R.H. the Duke of Cambridge, and many of the aristocracy. The college seems to be advancing in public opinion. We inspected the drawings, models, workshops, and other practical departments, which we are happy to say show considerable advancement in the knowledge of engineering. We were sorry on such an occasion, and in an Institution so promising, to see year after year, in the workshops, such schemes as the locomotive air-engine, and the imitation of Hero's rotary-engine; for in an establishment like the college, it is better to be too far behind-hand, than to be suspected of running after what savours of visionary projects.

LIST OF NEW PATENTS.

GRANTED IN ENGLAND FROM JUNE 24, TO JULY 18, 1848.

Six Months allowed for Enrolment, unless otherwise expressed.

Deane Samuel Walker, of London-bridge, merchant, for "Improvements in the manufacture of bands or straps for hats, caps, shoes, and stocks."—Sealed June 24.

Henry Archer, of Shaftesbury-crescent, Pimlico, Middlesex, gentleman, for "Improvements in matches, and in the production of light, and in the apparatus to be used therewith."—June 24.

William Hunt, of Dodder-hill, Worcester, chemist, for "Improvements in obtaining certain metals from certain compounds containing these metals, and in obtaining other products, by the use of certain compounds containing metals."—June 24.

Richard Clark, of the Strand, Westminster, lamp manufacturer, for "certain Improvements in gas burners, and in candle lamps and other lamps."—June 26.

Frederick William Mowbray, of Leicester, paper-dealer, for "Improvements in the manufacture of looped fabrics."—June 27.

John McIntosh, of Glasgow, gentleman, for "Improvements in obtaining motive power."—June 28.

Joseph Skertchly, of Anstey, Leicestershire, gentleman, for "Improvements in bricks, and in the manufacture of tobacco-pipes, and other like articles."—June 30.

Elizabeth Dakin, of No. 1, St. Paul's Church yard, London; widow, for "Improvements in cleaning and roasting coffee, in the apparatus and machinery to be used therein, and also in the apparatus for making infusions and decoctions of coffee."—July 3.

Nathaniel Beardmore, of 13, Great College-street, Westminster, for "certain Improvements in founding and constructing walls, piers, and breakwaters, parts of which improvements are applicable to other structures."—July 3.

John Martin, of Killyleagh Mills, Down, Ireland, manufacturer, for "Improvements in preparing and dressing flax, tow, and other fibrous substances, and doubling, drawing, and twisting flax, tow, and other fibrous substances, and in the machinery to be used for such purposes."—July 6.

Joseph Clinton Robertson, of Fleet-street, London, civil engineer, for "Improvements in the manufacture of gas." (A communication.)—July 6.

George Beattie, of Edinburgh, builder, for "an Improved air-spring and atmospheric resisting power."—July 6.

Anthony Lorimier, of Bell's-buildings, Salisbury-square, City, book-binder, for "Improvements in combining gutta percha and caoutchouc with other materials."—July 6.

William Edward Newton, of Chancery-lane, Middlesex, for "Improvements in the construction of stoves, grates, furnace or fire-places for various useful purposes."—July 6.

William Swain, of Pembroke, Hereford, brick-maker, for "certain Improvements in the construction of kilns for the drying and burning of bricks, tiles, and other earthen substances, and for the consumption of smoke and other noxious gases arising therefrom, and which latter improvements may be applied to all chimneys."—July 6.

Enoch Steel, and William Britter, of Lambeth, Surrey, manufacturers, for "Improvements in the manufacture of tobacco-pipes."—July 6.

Walter Orbel Palmer, of Southacre, near Swaffham, Norfolk, for "Improvements in machinery for thrashing and dressing corn."—July 10.

Richard Roberts, of Globe Works, Manchester, engineer, for "certain Improvements in, and application to, clocks and other time-keepers, in machinery or apparatus for winding clocks and hoisting weights, and for effecting telegraphic communication between distant clocks and places otherwise than by electro-magnetism."—July 11.

Leon Castelain, of Poulton-square, Middlesex, chemist, for "Improvements in the manufacture of soap."—July 11.

Felix Alexander Testud de Beauregard, of Paris, engineer, for "Improvements in generating steam, and in the means of obtaining power from steam-engines."—July 11.

Mathew Kirtley, of Derby, engineer, for "Improvements in the manufacture of railway-wheels."—July 11.

Jesse Ross, of Leicester, agent, for "Improvements in apparatus for dibbling and other agricultural puposes, part of which improvements is applicable to propelling vessels."—July 11.

William Edwards Staite, of Lombard-street, City, gentleman, for "Improvements in the construction of galvanic batteries, in the formation of magnets, and in the application of electricity and magnetism for the purpose of lighting and signalizing, as also a mode or modes of employing the said galvanic batteries or some of them, for the purpose of obtaining chemical products." (Partly a communication.)—July 12.

William Swain, of Pembridge, Hereford, brick-maker, for "certain Improvements in kilns for burning bricks, tiles, and other earthen substances."—July 18.

Jean Louis Lamenaude, of 30, Passage Touffroy, Paris, jeweller, for "a new process of applying or fixing letters of metal upon glass, marble, wood, and other substances."—July 18.

Charles Purnell, of Liverpool, dock-master, for "certain Improved apparatus to be applied to timber-loaded and other vessels laden with materials the specific gravity of which is lighter than water, preventing the necessity of abandoning them at sea by ridding them of the superincumbent water, and enabling them thereby to carry sail."—July 18.

William Edward Newton, of Chancery-lane, Middlesex, for "certain Improvements in machinery for letter-press printing."—July 18.

Joseph Stenson, of Northampton, engineer, for "Improvements in steam-engines and boilers, parts of which improvements are also applicable to other motive machinery."—July 18.

Joham Arnold Steinkamp, of Leicester-street, Leicester-square, Middlesex, gentleman, for "Improvements in the manufacture of sugar from the cane."—July 18.

CANDIDUS'S NOTE-BOOK,
FASCICULUS LXXXV.

> "I must have liberty
> Withal, as large a charter as the winds,
> To blow on whom I please."

I. Architects are apt to pay by far too little attention to locality, aspect, and other circumstances which influence the effect of buildings;—such, for instance, as distance—too little space for obtaining a satisfactory view from any point, or so much that it looks diminutive in comparison with what the design, whether shown in a model or a drawing of it (that is, an elevation), promises. Neither the model nor the elevation conveys, or can convey, the slightest idea of locality; and even if recourse be had to perspective and pictorial representation, the probability is, that there is a vast deal of imposition passed off under the plausible and innocent name of artistic liberties. Liberties of the kind are, however, sometimes carried so far as to amount to downright *lies*—an ugly word, it must be confessed; but the imposition so practised is a far more ugly thing. And surely it amounts to nothing less than a lie, although not a spoken one, to represent a front that will always be in shadow throughout the whole day, kindled-up and *illustrated* by sunshine, with all its details sparklingly touched and brilliantly brought out. In Mr. Blore's view of the new building at Buckingham Palace, the sun is made to shine from the north-east; which, not to call it a miracle, is at any rate a piece of great complaisance on the part of that luminary—a direct testimony to the all-commanding talent of that Mr. B. Such deceptions are, it would seem, not lies, but merely poetical and graphic fictions; and we ought to congratulate ourselves upon getting anything poetical at all, where the design itself is so terribly prosaic.

II. Locality has so very much to do with the actual appearance which a building makes, that unless some information be afforded in regard to it, we may be totally ignorant and unsuspicious of many circumstances that require to be understood. If a building has been seen, it is of course known how it is situated with respect to other buildings; whether in an open space or in a street; and if a street, whether it is a wide one or a narrow one; or if an open space, whether it is a regular or irregular one. It is desirable also to know what is its aspect, for it makes a very considerable difference whether a portico or colonnade faces the north or the south—the east or the west. Therefore, if a building is known to us only by means of plans and other drawings of that kind, confined to the edifice itself, we may form very erroneous ideas. Of course, such drawings acquaint us with its actual dimensions, but leave us in entire ignorance as to its relative size in comparison with adjoining or neighbouring buildings. It may chance, for instance, to be so greatly over-topped by them, that instead of answering to the prepossessing appearance which it makes when shown apart from other objects, as to make but a rather insignificant appearance in reality; consequently, much as the design may be admired, the structure disappoints when seen,—at least, at first, though it may recover our first good opinion afterwards. The reader knows the story of the lady who fell in love with a portrait, but was cured of her passion on finding that her Adonis wore a wooden leg. In order to judge what figure a building actually cuts, it is necessary to know something more about it than elevations, &c. show. The "*about it*" is here to be understood literally; since, with regard to effect, much depends upon accidents of site and locality—upon aspect, point of view, and various other circumstances, any one of which may be more or less influential, even taken by itself singly; therefore, when combined, must be so in a high degree, either favourably or the reverse.

III. In some stringent remarks that have just appeared upon the "Houses of Parliament," in another publication, it is hinted that it would not have been amiss had the excessive decoration of the river-front been considerably moderated, and the saving so effected been applied to the finishing-up another public and national edifice—namely, the pile of buildings which are termed collectively Somerset-place, whose now exposed west, or rather south-west side, makes a most mean, though not exactly beggarly, appearance—yet beggarly as manifesting the pinching poverty of a government which cannot afford to get rid of such an unseemly public exhibition of architectural *sans culottism*. It is no reproach to Chambers that he did not provide against an event which he could not possibly foresee. He would have been regarded as a madman, had he advised that the terrace-front should be returned and continued northward, in order to form a west façade that might possibly some time or other be exposed to view, on the side most of all favoured by aspect. He could no more have any idea that Waterloo-bridge would be erected, than that such architecture as King's College would be brought so closely into contact, as to be made to appear part of his design. The bridge has certainly rendered one service to Somerset-place, since it enables us to obtain a view of the river-front and its terrace. But the formation of the street leading to the bridge has laid open to sight what was never meant to be seen at all, and therefore now detracts sadly from the grandeur of the façade which is beheld in connection with it. The disparity between the two is nothing less than offensive. The feeling it produces is akin to that which we should experience on beholding a costly sideboard and a kitchen-dresser cheek-by-jowl in a dining-room. The juxta-position which here presents itself is not like that of "pearls upon an Æthiop's arm," but of a blooming face upon the shoulders of a Blackamoor,—the head of Hebe upon the shrivelled carcase of a hag;—in short, we perceive Chambers and Pecksniff arm-in-arm together: no disrespect is intended to the former, he being sufficiently excused by what has been stated above. Still, that is no excuse for our neglecting to do what the greatly-altered locality of the building renders so highly desirable. If it is not worth while, merely for appearance-sake, to bestow a decent architectural exterior on the exceedingly unsightly and disfiguring range of building alluded to, how can we reconcile ourselves to the enormous outlay for mere embellishment in the river-front of the Houses of Parliament; and which, after all, does not produce any corresponding degree of effect? Either the "Houses" accuse us of the most niggardly meanness for allowing Somerset-place to remain in the disgraceful and unfinished state which it is; or Somerset-place accuses us of absurdly wanton extravagance, for crowding such a profusion of minute details and carved-work into that front of the "Houses" where they are quite lost, and where a few boldly-touched finishings would have told quite as well—perhaps even very much better. The west wing of Somerset-place, on the contrary, though at present a most offensive eye-sore—and perhaps doomed to remain so—is most admirably situated for architectural display. There is not any spot in the whole metropolis which affords so many of the requisites for a fine architectural scene. That side of the buildings of Somerset-place is favoured, not only by aspect, but by due space before it—neither so great as to take off from the size of the building, nor insufficient for viewing it as a whole. To these advantages, other peculiar and accidental ones may be added: the building descends so much below the level of Wellington-street, or the roadway leading to the bridge, that the street becomes as it were a terrace or elevated staging, from which it shows—that is, would show itself, most picturesquely, were it a worthy piece of architecture, instead of being the abomination it now is. When looked down upon from the parapet of the road, it would be seen rising statelily from a deep substructure. And that west front and the river-front—which latter shows at present only as a mere mask—seen together, would produce a fine palatial mass, and in combination with Waterloo-bridge, a most striking and noble group of architecture. The river-front itself, which now looks only like the fragment of an unfinished design, would gain in importance by becoming a portion of one consistently grand design. Besides all which—but let us have done with "besides's." We have "Marble Arches," and "Nelson Columns," and "Wellington Statues," and a good many other things that shall be nameless, to show how freely tens-of-thousands have been flung away on very futile if not absurd objects. Let us then have at least one public edifice that will serve as a monument of our notable economy and frugality. Therefore, since so it must be, let it even be Somerset-place. Let the foreigner who gazes with astonishment at such an achievement by individuals as Waterloo-bridge, look with astonishment of a different kind—with scornful wonder at the paltriness that is allowed to disfigure and disgrace a government building which might, with comparatively little cost, be rendered a uniformly noble pile.

IV. My good friends, the architects,—the friendship between us is not, perhaps, of the most cordial kind, my own share being marked more by boisterous freedom than by compliments and courtesy—still I say my very good friends, the architects, may be said to have contrived to outwit and enslave themselves. So long have they gone on preaching up the accursed and superstitious doctrine of its being unlawful to deviate from established forms and proportions, or from the precise letter of mechanic-enacted rules, that even those who could do so successfully, dare not even attempt it, so greatly do they stand in awe of the sneers of the whole tribe of

Incapables, who scout the idea that it is possible for any other than hackneyed forms to be beautiful, and who flatly deny it to be in the power of artistic conception to produce what, though different in character, should be equally beautiful in its peculiar way. Unhappy architects! ye have locked yourselves in, and flung away the key that would open the door and send you forth—the worthy and really gifted among you rejoicing once more in liberty and light. Unhappy architects!—unhappy architecture, also! for thou art now fettered, and delivered over to the tender charge of thy jailor, PRECEDENT!

V. Precedent is adhered to even for that lawless, hybrid style which we denominate Elizabethan. No one, indeed, has yet attempted to draw up a formal code of rules and regulations for it—perhaps, because it would be a task of very great labour to attempt to systematize so chaotic a mass as the various examples of it constitute; any one of which, however eccentric and exceptional, may now be quoted as sufficiently valid authority. This must be allowed to have its convenience; no more being required than to follow Precedent merely piece-meal, and patch up a design out of odds-and-ends, taken at random, and put together without any regard even to general consistency of composition. The name "Elizabethan," at once sanctifies all absurdities and all crudities: it operates as a talisman and an ægis against the shafts of criticism. It is in vain to object to that offensive inequality of design which results from plainness and coarseness in some features, and studied ornateness, perhaps finicality, in others. The greater the incongruity, the greater, it would seem, is the *Elizabethan-ness*. The utmost and most convenient latitude is freely allowed: forms and features strongly partaking of the latest-expiring Tudor style may be mixed up *ad libitum* with ultra-Italianized ones; no matter how incoherently, when incoherence is thought to belong to, if not actually to be the very genius of the style itself. Although for a very different reason, Elizabethan possesses one great recommendation in common with the pure Grecian style. What it is you shall learn by-and-by—that is, in my next Fasciculus; till which appears, I leave you to cogitate upon the matter, and solve the riddle, if riddle it be, for yourselves. Or, should your curiosity evaporate in the interim, it will not greatly matter. Putting, like a frugal hostess, that dainty bit "by for supper-time," what I now say is, that we do not at all avail ourselves as we might do of the opportunity which Elizabethan architecture holds out to us,—of the convenient pretext which it affords us for working out a style founded upon the *Italianism* or early *Cinque-cento* ideas which, although they show themselves only here and there partially, and more or less imperfectly, strongly mark those examples in which they occur to any extent. In reverting to Elizabethan, we have considered it rather as being the latest stage of expiring Gothic, than as the incipient one of a quite different architectural system; which, had it been allowed to proceed as it originated, and to develope itself freely and naturally out of its first rudimentary shoots, instead of being put into the hot-house of Palladio and Jones, would doubtless have produced other blossoms and fruits than those which have attended such "forcing" system. When Elizabethan was recommended as the style most of all suitable for the new Houses of Parliament, people—and architects among the rest, were puzzled to know what was to be understood by such name. Was it to be interpreted literally or liberally? If the former, it of course excluded every style that had been used before or up to the period of Elizabeth's reign, unless Elizabethan and Ante-Elizabethan mean one and the same thing, and unless Elizabeth reigned before she was born—a mystery I willingly leave to wiser heads than mine. On the other hand, if Elizabethan was to be interpreted liberally and latitudinarially, it would seem to imply that the style to be adopted might be either that which was actually employed during the latter half the 16th century, or which afterwards came up in the earlier part of the following one; and the way for which had been opened by the influences and tastes of the previous Elizabethan period. It is perhaps just as well that the taste of Elizabeth's grandfather—at least, that of his age—was preferred to that of her own. Nearly all the competitors for the "Houses" eschewed Elizabethan, and no doubt very prudently; for it would, perhaps, have occupied them till now to elaborate out of it aught that would have been both worthy of and suitable for the occasion.

VI. Some are so very sensitive and captious about terms and names, that I wonder they do not affect to be scandalized at that of the "Lancet style." It cannot, indeed, be said to be a very blunt one; on the contrary, it is sharp enough—so sharp as to pierce an ear that is at all refined. It must surely have been invented by Dr. Sangrado, or other practitioner of phlebotomy. Or it must have originated with the Company of Barber-Surgeons, so truly *barberous* is it in sound. If not so, it must have been applied in sneering derision by some such critics as those who make themselves merry with Nash's *extinguisher* in Langham-place, and Barry's *dumb-waiters* in Trafalgar-square. Lancet-style!—horrible name; suggesting ideas of bloodshed, at least of blood-letting! Let it be reformed by all means; more especially as people are now endeavouring to substitute more appropriate terms for those introduced by Rickman, notwithstanding that they have obtained general currency among as. If "Perpendicular" is to be transformed into "Rectangular" style, surely "Lancet" will no longer be tolerated by "ears polite," or ears archæological. Let a General Council of Archæologists be held forthwith; let the Institute assemble in solemn or somnolent conclave, to devise some less odiously vulgar name for what is now called the Lancet style. Should they not be able to think of one, still, they might possibly *dream* of one; or else they go to sleep to very little purpose indeed.

VII. Plans, elevations, and sections of Stowe, would just now possess considerable interest, even for those who would not care for them at any other time. None, however, are in existence,—at least, no published ones. There is no work which so describes that princely mansion;—no plates of it in any of the several collections which assume the title of "Vitruvius Britannicus," although they all contain subjects of far inferior note, and inferior merit also, and more than one which has nothing whatever to recommend it. Almost all that is known of the architectural authorship of Stowe is, that the original house is said to have been designed by Viscount Cobham, and the additions, comprising the stately south front, by the first Lord Camelford (about the year 1775). Vanbrugh and Kent designed some of the garden-buildings; but of the professional architects employéd upon the mansion itself, the only names which appear to be known are those of Borra and Valdrè, both foreigners. Stowe is infinitely too modern to excite any sympathy among the admirers of ruins and rubbish. When it becomes an antiquity, and thereby entitled to the epithet of "venerable," it may acquire that value in the eyes of posterity which it has not in our own. And let us hope that it will be permitted to remain; and not doomed to be taken down and sold piecemeal, as were both Wanstead and Worksop. We can ill-afford to spare such an example of a palatial English residence, now that nothing else of the same class and character is erected. Scarcely a mansion of any architectural note at all has been erected for many years. What few large country-houses have been built, mostly affect antiquity, without either the charm or the merit of it. The tide now runs in favour of Model Lodging-houses and Buckingham Palaces,—of Baths and Washing-houses: but Greenwich Hospitals, and Blenheims and Stowes, are quite beyond our mark. Neither the aristocracy of rank nor the aristocracy of wealth patronise such architectural grandeur as marks the last-mentioned piles. Let it not be supposed from this, that unqualified praise of either Blenheim or Stowe is intended. In both of them there is much to censure—a great deal that might be many degrees better: still, there is the magnificent and the noble—which, as things now go, is a very great deal indeed. As to the *grandiose*, where will you now find it? Go and look for it in the façades of the British Museum or Buckingham Palace; and if you can discover it there, you may congratulate yourself as being able to discover the longitude also.

NOTES ON ENGINEERING.—No. X.

By HOMERSHAM COX, B.A.

The Dynamical Deflection and Strain of Railway Girders.

There is no subject in practical science which has been more elaborately investigated than the theory of the statical transverse strength of beams. It has fortunately happened that two different classes of investigators—mathematicians and experimentalists—have cooperated in the research: and the result of their united labours has been a valuable and comprehensive system of knowledge.

But the DYNAMICAL strength of beams, or their capability of sustaining weights moving rapidly over them, has never been satisfactorily discussed. There does not appear to be extant a single theoretical investigation of this subject—and the deficiency is due to two causes: it occurs partly because the subject has but comparatively recently grown into importance; partly because of its excessive and insuperable difficulties when investigated by the exact methods of theoretical mechanics. The following paper is a contribution to a more accurate knowledge of this important question, which has at length attracted the attention due to its influence on the security of railway traffic. The necessity of further inquiry seems to be generally acknowledged among engineers; and by the recommendation of the Commissioners of Railways, in a published minute of the 29th of June 1847, a government commission has been appointed for the very purpose. The minute expresses a doubt "whether the experimental data and the theoretical principles at present known are adequate" for the "designing iron bridges, when these are to be traversed by loads of extraordinary weight at great velocities."

There seems to exist great discrepancy of opinion as to the effect of the velocity of transit. Some have imagined that it may become a source of safety, by causing the railway train to pass over before the girder has had time to yield. Others, again, have estimated the effect of the moving load as highly as six or seven times that of the same load at rest. In the following investigation, both these opinions will be shown to be incorrect: they are here cited merely as indications of the extreme uncertainty prevalent on the subject.

The method of inquiry about to be explained consists, not in determining the dynamical strain absolutely, but by comparison with the corresponding statical strain. The results will consequently be much simpler than they would be if the dynamical strain were estimated independently. The deflection which a given load *at rest* upon a girder produces, will be always taken as one of the known data of the problem. The determination of this statical deflection, as it forms the basis of all the remaining calculations, is the first point of inquiry.

When a beam is not affected by a permanent set or defect of elasticity, it appears, both from theory and actual experiment, that the deflection by a weight resting at its centre is very nearly proportional to that weight—that is, if a given number of tons deflect it one inch, double the number of tons will deflect it two inches. This result is arrived at by Professor Moseley in his "Mechanical Principles of Engineering," and M. Navier in his "Resumè de Leçons de Construction," by independent methods. Its near accordance with practical truth has been abundantly confirmed by experiment, as may be verified by reference to numerous published accounts of actual observations on the subject, and especially to Mr. Hodgkinson's invaluable "Experimental Researches on the Strength of Cast-Iron." This work gives the results of an exceedingly large number of experiments, made by the author and others, on the transverse strength of beams loaded at their centres; and although these beams were of very different forms and dimensions, the law indicated is nearly observed in all of them. Whether the section of the beam be rectangular, triangular, or **T**-shaped, with the vertical rib either upwards or downwards, the constant ratio, in each beam, of each deflection to the corresponding load is nearly maintained: and the same remark applies to beams of the form most useful for railway purposes—that of an upper and lower flange connected by a vertical rib.

It will be found, however, by reference to the tables in Mr. Hodgkinson's work, that the actual deflections are somewhat more than the theoretical law would make them. This discrepancy may be accounted for by attributing it to the defect of elasticity, which the ordinary theory of beams does not consider. As this defect is not generally very great, it will here in the first instance be neglected: the deflections will primarily be estimated as if the elasticity were perfect; and subsequently the modifications due to defect of elasticity will be taken into consideration.

Work Done on the Deflection of a Beam.

The "work done" by a moving force may be defined to be the product of that force into the distance through which it acts. A familiar instance of the use of this measure is the Steam-Engine; where the work done receives the particular name of Horse-Power. If the pressure on the piston were uniform, that pressure (in pounds) multiplied by the distance through which it is exerted (in feet) would, if divided by 33,000, give the horse-power. But in the steam-engine, and all other practical instances, where the pressure is not uniform, but varying, it is impossible to calculate the work done by this direct multiplication. Where the value of the moving force is constantly altering, we may resort to either of the following methods of ascertaining the work done by it,—we may multiply its *average* value by the distance through which it acts; or, when that average cannot be ascertained, we may consider the whole distance divided into elementary portions, so small that it may be supposed without sensible error—that the pressure is at least uniform while it acts through each portion in succession. The aggregate work done, is the sum of the work done on each of these portions—that is, it is the sum of the products of each portion of the distance and the corresponding pressure.

This process of summation, when carried out with the greatest possible accuracy, is equivalent to that of mathematical integration; in which case, the work done by a varying pressure may be defined, in mathematical language, to be the integral of the product of the pressure, and its "virtual velocity." The work done in deflecting a beam by pressure at its centre is easily ascertained, if that pressure be assumed proportional to the deflection. Calling the deflection x, and therefore the pressure αx (where α is a constant depending on the dimensions &c. of the beam) we have—

$$\text{work done} = \int_0^x \alpha x \, dx = \frac{\alpha x^2}{2} = \frac{\alpha x}{2} \cdot x.$$

Now $\frac{\alpha x}{2}$ is the pressure or weight which would statically maintain half the deflection x. Hence, *the work done in producing a given deflection is equal to the weight which would statically maintain half the deflection, multiplied by the whole distance of deflection.*

The value of this rule will appear hereafter.

Distinction of Gradual and Instantaneous Loading.

When experiments are made on the strength and deflection of beams, they are generally loaded very gradually at their centres. Each addition to the load is allowed to produce its full effect before more be imposed. Consequently, at every stage of the experiment, the beam is in a state of statical equilibrium: the pressure of the load on the beam is always just equal to its weight, and is never increased by any momentum arising from downward velocity.

But if the whole load be suddenly and at once placed on the beam, while it is as yet undeflected, the effects are entirely altered. The deflection is greater than the same load would produce if gradually applied: for when the beam has reached the point of statical deflection, the momentum acquired by the downward motion urges it further; and the descent of load continues till it be *brought up* (so to speak) by the increased resistance of the beam. Afterwards, the beam and load rise again, as the deflection has been carried beyond the degree at which it can be statically maintained.

In the case here supposed of instantaneous loading, nothing like impact or sudden collision occurs. The pressure at the centre of the beam is finite and continuous. The load does not *fall* upon the beam—it is merely supposed to be placed originally in close contact with the beam, and then suffered to instantaneously rest upon it.

For the sake of elucidation, one or two instances of analogous action may be cited. If a common balance have its fulcrum above the points of suspension of the scales, and a weight suddenly rest in one of the scales, the lever will turn through a much greater angle than if the same weight were applied in small successive portions.

If an elastic string suspend vertically a weight from one end of it, the string will be more stretched if the whole weight be suffered to act at once, than if applied in small portions. It will be found, that if the extension of the string be proportional to the stretching force, the extension produced by the descending weight will be *twice* that due to the gradual effect of the same weight.

A light cylinder of wood, loaded at its lower end, and floating vertically in water, furnishes another illustration. If the cylinder be raised a little above its position of equilibrium and then let go, it will sink twice the distance it has been raised, if the motion be so small that the resistance is equal to the hydrostatical pressure.

In the same way, in a perfectly elastic horizontal beam, loaded at its centre, the effect of instantaneous loading is double that of gradual loading. For, by a known principle of mechanics, when a material system moves from one position of rest to another position of rest, the work done by the retarding forces is equal to the work done by the accelerating forces. For any small deflection of a beam by instantaneous loading, its position of ultimate deflection is one of instantaneous rest, for immediately before it arrives at that position, all the parts of the beam descend, and immediately after, ascend. Also, the work done by the accelerating force is the weight actually resting on the beam, multiplied by the space of deflection: and the work done by the retarding forces is, by what has been said above, "equal to the weight which would statically maintain half the deflection, multiplied by the whole distance of deflection." Therefore, putting the two amounts of work done equal to one another, we see that the weight actually upon the beam is that which would statically maintain half the deflection. In other words, *the deflection is doubled by instantaneous loading.*

Transit of a Single Weight.

We now proceed to examine the effect of the transit of a single weight *along* the girder, and first of all to show that its effect cannot exceed that which it has just been estimated to produce, if stationed at the centre of the girder and allowed to descend freely from the undeflected position—in other words, it will be proved that at whatever rate the weight may travel over the girder, its ultimate strain and deflection cannot be more than double the corresponding statical effects produced when it *rests* at the centre of the girder.

There is a general rule of constant use in engineering which, expressed in practical language, states that power is never gained, but only modified, by the intervention of machinery. This rule may be more scientifically expressed and extended by tracing it to its origin—it is a particular case of the principle known in theoretical mechanics, as the Conservation of *Vis Viva*. This principle may be very conveniently enunciated by employing the term "work done," as defined above: and it then assumes this form of enunciation—that the *vis viva* gained or lost by a system in moving from one position to another, is equal to twice the difference between the work done by the accelerating, and that done by the retarding, forces in the same interval.

From this it follows, that where there is *no* gain or loss of *vis viva*, there is *no* difference between the work done by the accelerating and retarding forces respectively. Hence, if the parts of the system be moving at the same velocity in the second position as in the first—or if both positions be positions of rest—the aggregate work done in the interval by the retarding forces is equal to that done by the accelerating forces.

A very simple case will illustrate this theorem. If a locomotive-engine travel a mile along a railway, and its velocity at the end of the mile be the same as at the beginning of the mile, the work done by all the forces which have resisted its motion is in the aggregate just equal to the horse-power developed in the steam-cylinders. And this equality holds good, however the engine have moved in the interval—whether on a straight level road, or on severe curves and gradients—whether the speed were uniform or very irregular—whether the steam were on the whole time, or the engine during large parts of the journey moved by its momentum only. The intermission of the moving force and all other irregularities disappear in the result. To establish equality between the work done in moving, and that done in retarding, the engine, all that is necessary is that the engine be moving neither faster nor slower at the end of the mile, than at the beginning of it.

Another illustration will serve to show the extreme generality of the principle in question. If a certain quantity of water have to be raised a certain height, the amount of work actually requisite for effecting the object is in all cases equal to the weight of water multiplied by the vertical height. This amount of necessary power or work is incapable of being diminished by any mechanical or hydraulic contrivance. The water may be contained in a vessel which is drawn up perpendicularly, as from a well, or which is drawn up an inclined plane or by a spiral path; or the water may be raised by an Archimedian screw, or by buckets attached to the periphery of a revolving wheel, or by a hydraulic-ram, or by a force-pump; or lastly, it may be thrown up in a jet, as from a fountain or fire-engine. But it is physically impossible, by these or any other methods, to diminish the requisite amount of labour. It is, of course, easy to increase the amount by a waste or unprofitable expenditure of labour, such as is caused by friction of the machinery, or the mutual action of the particles of water among themselves. But supposing no waste of force to occur—supposing all the power usefully employed in simply raising the water without doing anything else; then the amount of that power is in all cases just what has been stated—the weight of water multiplied by the vertical distance through which it is raised.

The rule is of universal application, and there is no other principle of dynamics of such great and constant utility in practical science; for it embraces all those cases of motion with which the engineer happens to be concerned—cases where the motion either ceases, or has the same values, at regularly-recurring intervals.

The case before us, of the transit of a weight along a girder, is a striking exemplification of this Principle of the Conservation of Work. For this principle enables us immediately to compare the effect of a weight moving along the girder, and that of the same weight stationed at its centre, and descending. If the deflection be the same in both cases, the work done by the descent of the load in both cases is the same—namely, the weight multiplied by the vertical descent: and this is true, whatever be the path of descent. Now, it has already been shown, that in the case of instantaneous loading, the work done by the descent of the weight is equal to that necessary to produce in the beam the deflection which twice the weight would statically maintain. Hence, the travelling weight can do no more.

The value of this conclusion appears the greater, when it is considered that it avoids all hazardous hypotheses as to the forms assumed by the beam during the transit. However the beam may be bent—whatever may be the nature of its vibrations and internal action, this is certain,—that when its elasticity is unimpaired, a weight travelling along it cannot, under any circumstances whatever, more than double its corresponding statical deflection. To suppose it capable of doing more, is to suppose the physical impossibility of a gain of power.

But though the travelling weight cannot, under any circumstances, produce *more* than double the statical deflection, it is quite possible that it may do *less*. A large portion of the work done by the weight may be absorbed in producing lateral vibrations and other irregularities of motion in the beam. All these concomitant operations act by way of diminution, and tend to make the dynamical deflection *less than* double the statical central deflection.

In determining the actual amount of this diminution, the velocity of transit must be taken into account. For that there is some particular velocity for which the deflection is a maximum, is obvious from this simple consideration—that when the weight travels exceedingly slowly along the beam, it always exerts a statical pressure, and does not tend to increase by momentum the deflection beyond its statical amount;—and, on the other hand, when the weight travels with excessive rapidity, it may not have time during the transit, to sink even the distance of statical deflection. To take the limiting case, when the velocity is indefinitely great, the descent of the weight must be indefinitely small; for even if it fell freely, and there were no beam to support it, the distance of descent in an indefinitely short time is inappreciable.

Effect of the Inertia of the Beam.

There is, then, between the exceedingly high and the exceedingly low velocity, some particular intermediate speed which produces the greatest possible deflection. Before, however, considering what that velocity is, or endeavouring to establish a direct relation between the velocity and the deflection, it is necessary to examine more particularly the case just referred to—where the velocity of transit is so great, that the weight has not *time* to sink beyond a certain degree.

Now, there are two ways in which this consideration of time might be supposed to affect the amount of deflection. The first is that already stated, where the period of transit is so short, that even if the weight descended freely, without support from the beam, its descent would be inconsiderable. This case may, however, be at once excluded, when it is considered that at all practicable railway velocities, the time of transit over a long girder (50 to 80 feet) could not be much less than one second, that a body would fall freely upwards of 16 feet in that time, and that its actual descent (equal to the deflection of the girder) is only a few inches.

But there is another way in which the consideration of time might be supposed to affect the deflection: there might not be time enough to overcome the inertia of the beam. This case requires more particular examination.

A person skating over a weak piece of ice may sometimes, by moving rapidly, glide over it safely before it have *time* to break—that is, before the pressure of his body have impressed on the ice the downward motion sufficient for it to attain the point of fracture while he is passing over it. Now, by the general principles of mechanics, the same pressure which, acting for a given time, would produce a great velocity in a small mass, will produce proportionably little velocity in a large mass. In order then that the inertia of the ice may, in the case supposed, be a cause of safety, it must be large in comparison with the pressure acting on it; that is, the mass of ice acted upon must greatly exceed the mass of the man's body.

In the same way, in order that the inertia of a girder might be a cause of security, the mass of the girder must be very much greater than that of the train passing over. But it will be shown that the mass of the former does not, for heavy loads, exceed that of the latter so greatly as to perceptibly diminish the deflection. It has sometimes been found useful to add to the inertia of the girder by laying on it heavy ballast, and by this means the structure is rendered *steadier*,—that is, the slight lateral oscillations and other irregularities of motion are reduced. But it is only these smaller or subsidiary movements that can be diminished by adding to the weight of the girder. Its mass, and that of the permanent load upon it, is not in generalso large as to materially influence the main, or vertical, deflection, when produced by nearly as heavy a load as it will safely bear.

When the train passes over the girder, the centre of gravity of the whole system sinks, the impressed moving force downwards being the weight of the train, and the motion of the centre of gravity being retarded by the elastic force of the girder.

To take a case every way unfavourable to the conclusion which we wish to establish, let the greatest deflection be 3 inches, and the velocity of the transit so great that the weight passes over the girder in a second of time. This would be the time of transit over a girder 88 feet long, at the rate of a mile a-minute. Now, the extreme deflection may be supposed to be accomplished in half the time of transit, or the centre of the girder sinks 3 inches in half a second. The centre of gravity of the whole system at no time sinks so much as the centre of the beam sinks, for its two ends do not sink at all. On the whole, it seems an ample allowance to suppose the maximum vertical descent of the centre of gravity of the beam 1½ inch. Now, to find the work which would *alone* produce this velocity, we must have an equation of *vis viva*, excluding the retarding force.

By the ordinary rules for calculating the rectilineal motion of bodies, if a given mass M originally at rest be acted upon at its centre of gravity by one uniform force f moving through a space x in the time t,

$$2\,\mathrm{M}\,x = f\,t^2.$$

Suppose the force to be that of a small weight. The mass of this weight will be found (on substituting numerical values and putting gravity = 32) to be only the 32d part of M, if the latter move through 1½ inch in half a second.

The beam is usually constructed to bear a pressure considerably exceeding its own weight. In that case *less* then one 32d of the work actually exerted by the travelling weight would suffice for the mere acceleration of the beam: and we come to the conclusion, that even at the highest practicable velocity, the power required to set the beam in motion subtracts very little of the power producing deflection. In other words, when the mass of the load is not small compared with that of the beam, the deflection is never materially influenced by the inertia of the beam.

Influence of the Velocity of Transit on the Deflection in the case of a Single Weight.

Having arrived at the important conclusion that when the travelling weight is large, the inertia of the beam is an immaterial consideration, or that the effective moving forces are inconsiderable compared with the impressed forces, we might suppose the mutual pressure between the beam and the weight statically equal to the force which the former by its elasticity exerts in an upward direction to resist deflection.

But, in fact, the mutual pressure between the beam and the weight is an unknown force, not generally susceptible of exact determination. During the first part of the motion, the weight does not, so to speak, exert its full pressure on the beam, for the surface yields and recedes before it. During the latter part of the descent, on the contrary, the pressure in question exerts a superior power, to destroy the momentum previously acquired by the descending weight. The weight then moves downwards, first with an accelerated, and subsequently with a retarded, velocity: or the pressure on its under side is in the former stage of motion, less, and in the latter stage greater, than the effect of gravity.

The path of the weight is likewise unknown, for the motion is made up of two parts—the motion along the beam, and the motion of the beam itself. If, indeed, it be assumed that the motion is always along the beam, or that at every instant the curvature of the beam has, at the point of mutual contact, the same tangent as the path of the weight, the problem would be capable of solution. The investigations of Professor Moseley and M. Navier have determined the curvature of the beam sufficiently to afford means of tracing the curve described by the moving weight; and therefore its pressure, which is equal to its centrifugal force + the effect of gravity, might be ascertained.

The hypothesis which would lead to these results is, however, arbitrary and unsafe: and besides, the curvature of the beam as mathematically determined, is not exactly that which occurs in actual practice, where the elasticity is always more or less imperfect. The difficulty is however of no great importance, because, as will be presently shown, it does not occur where the moving body is not a single weight, but a long train. And the subject is here referred to, merely to show the almost insuperable difficulties of determining the motion of a single weight along an elastic beam.

Uniformly Distributed Load.

We have hitherto considered the effect of a single weight pressing only at one point of the girder. The more important practical case, where the pressure is applied to a considerable surface, remains to be examined.

In entering upon this inquiry, the consideration of horizontal motion will be in the first instance excluded. The distinction between the effects of gradual and instantaneous loading has been already pointed out, in reference to a single weight; and the comparison may now be extended to the case of an uniformly distributed load. If this load be gradually laid on, it produces less deflection than when laid on all at once. A series of weights applied simultaneously all along the undeflected girder, will move vertically downwards, and acquire momentum which has to be destroyed by an increased exertion of the elastic forces of the girder. In this case, as in that of a single weight, the ultimate deflection and pressure will be doubled, as will be demonstrated by analogous principles.

The beam being, as before, supposed to be perfectly elastic, the central deflection is proportional to the weight of the uniformly distributed load. If a be the length of the beam, x the central deflection, u the weight of a unit of length of the load, we may put $u\,a = \alpha\,x$, where α is a determinate constant.

Let x', y' be co-ordinates of any point in the surface of the beam, which is supposed to have the same curvature as its neutral axis. Then as the curvature is always exceedingly small, $u\,d\,y'$ is the weight of an element of the load, and $u\,d\,y'\,d\,x'$ represents the product of this weight and its virtual velocity, when the centre of the beam is displaced through a small vertical distance $d\,x$. The product of all the pressures and the corresponding virtual velocities is equivalent to

$$\int u\,d\,x'\,d\,y',$$

taking y' between the limits 0 and a. It may mathematically be shown that this integral is equal to $u\,a\;d\bar{x}$, where $\bar{x}$ is the vertical ordinate of the centre of gravity of the load. Also, from the equations given by Professor Moseley, in his "Principles of Engineering," Art. 374, it may be shown by a simple process, which is here omitted for the sake of brevity, that $\bar{x}$ is proportional to x, and may therefore be put $= \beta\,x$, where β is a determinate constant.

$$\text{Hence, } u\,a\;d\,\bar{x} = \alpha\,\beta\,x\;d\,x.$$

The integral of this, between limits 0 and x, is the total work done in producing the central deflection. This integral is equal to

$$\alpha\,\beta\,\frac{x^2}{2} = \frac{\alpha\,x}{2}\,.\,\bar{x}.$$

Or the "work done" is equivalent to that produced by a weight which would statically maintain half the deflection, moving through the whole space which the centre of gravity actually described. Hence, by the same reasoning as applied to the case of a single weight, *the statical deflection and pressure are doubled by instantaneous loading.*

Transit of a Continuous Load.

By combining the conclusion just arrived at with the principle which has here been termed the Principle of the Conservation of Work, it is readily seen that the statical strain and deflection cannot be more than doubled by the transit, at any horizontal velocity, of a uniform load of the same length as the girder. Indeed, the dynamical will in general be considerably less than double the corresponding statical effects.

It has been shown that where the weight of the load sustained is nearly as great or greater than that of the beam, the force required to produce motion in the beam is inconsiderable compared with the actual deflecting forces. The beam itself, therefore, is then always nearly in a state of equilibrium, and its form nearly the same as that which would be statically produced by the external pressures. If this be assumed to be strictly true, it follows that the curve of deflection is concave in every part, and therefore that no part of the beam sinks while another part is rising—that *all* the parts sink together, and all rise together. The vertical motion is so extremely small and gradual, that there can be no danger in assuming that all the parts arrive at their lowest positions at the same instant. It follows then, as previously to that instant the motion was downwards, and subsequently upwards, the beam in its lowest position is at rest, either instantaneously or for a definite period.

In this position of rest, the pressures on the surface of the beam are in statical equilibrium with its internal forces. At the same time, the pressure produced by the travelling load is the same as if the curve of deflection were a *fixed* curve.

Effect of Centrifugal Force.

When a body, moving along a fixed curve, is acted upon by no forces but the pressure of the curve and its own weight, the pressure on the curve (by the known principles of mechanics) is equal to the centrifugal force, plus the normal component of the weight. The curvature of a deflected girder is in general so exceedingly small, that it will be quite safe to assume the pressure equal to the centrifugal force, plus the weight itself. The curve assumed by the surface of the beam depends on the forces acting on it; and we here suppose the beam to be at *rest*, although the load upon it is in motion. Hence, the elastic forces of the beam are in *statical* equilibrium with the pressures on the curve.

The origin of co-ordinates being at one end of the beam, and the axis of x, measured vertically downwards, at any point (x, y) of the curve, the tangent of the angle of horizontal inclination is $\frac{dx}{dy}$, which is always very small. Hence, neglecting the square of that quantity as inconsiderable compared with unity, we may put the inverse of the radius of curvature at the point $(x, y) = -\frac{d^2x}{dy^2}$. (The sign never changes, as the curve is everywhere concave upwards.)

From the theory of perfectly elastic beams, it appears that $\frac{d^2x}{dy^2}$ is, at every part of the beam, proportional to the moment about (x, y) of all the pressure acting between that point and either extremity of the beam. Here, the pressures between (x, y) and the origin are the centrifugal forces and the weights acting downwards, and the pressure of the abutment acting upwards. The moment of all the centrifugal forces may be first ascertained.

Each small portion of the load may be supposed to act independently of the rest, or to press on the curve with its own weight and its own centrifugal force. Let m be the unit of mass; and therefore, at any point $(x' y')$ intermediate between (x, y) and the end of the beam, $m\, dy'$ the mass of an element of the load. Calling V its linear velocity, it appears from what has been already said about the radius of curvature, that $V^2 \frac{d^2x'}{dy'^2} m\, dy'$ is the centrifugal force of that element. The moment of this centrifugal force is $-V^2 \frac{d^2x'}{dy'^2} \cdot (y-y')\, m\, dy'$.

The moment of all the centrifugal forces about point (x, y) will be found by integrating this expression between the limits $y' = 0$ and $y' = y$. So it may be ascertained that the moment of these forces is

$$m V^2 (y \tan \beta - x),$$

where β is the horizontal inclination of the curve at the origin. $y \tan \beta - x$ is the length of a vertical line drawn downwards from the point (x, y) to meet the tangent drawn from the origin; and is very small.

The weight of the portion of the load upon the horizontal length of the beam y, is $m g y$; and its moment about the point (x, y) is the same as if the weight were collected at a point half way between (x, y) and the origin, and therefore equals $\frac{m g y^2}{2}$.

Also, if P be the pressure of the abutment, $P y$ is its moment; and representing the constant, by which the radius of the curvature has to be multiplied to render it equal to the sum of the moments, by E I, we have—

$$E I \frac{d^2x}{dy^2} = m g \frac{y^2}{2} - P y + m V^2 (y \tan \beta - x).$$

This equation is integrable in its present form; but as the last term of it is very small, we may make an alteration which will tend very much to the simplicity of the results. The centrifugal pressures cannot under any circumstances be great compared with the other forces, as may readily be foreseen by considering that in all cases of actual practice, the curvature is very small on account of the very small proportion which the central deflection bears to the length of the beam. *For any central deflection previously assigned*, the curve would be very little altered if the centrifugal pressure were uniformly distributed. Therefore, in the above equation, the small term $m V^2 (y \tan \beta - x)$ is neglected in estimating the radius of the curvature merely. Now, it appears from the "Mechanical Principles of Engineering," Art. 374, that when the beam is subject to any uniformly distributed pressures whatever,

$$\frac{d^2x}{dy^2} = \frac{24\,D}{5a^4} \left(\tfrac{1}{2} y^2 - a y\right),$$

where a is half the length of the beam, and D is its central deflection. The curvature of the beam, when it assumes its permanent form under the influence of a passing load, will not greatly differ from that which this equation indicates. Of course, this hypothesis does not suppose the distribution of the centrifugal pressures to be actually uniform—it merely presumes that the curvature of the beam, *for a given deflection*, is nearly the same as if the pressures were so distributed.

From the equation last given, the value of $\frac{d^2x}{dy^2}$ at the centre, is $-\frac{24\,D}{10\,a^2}$. Therefore, at the centre, a weight moving with velocity V, has a centrifugal pressure $= V^2 \cdot \frac{24\,D}{10\,a^2}$ times its mass.

To ascertain the whole effect of centrifugal pressure, we have evidently the expression

$$-\int m V^2 \frac{d^2x}{dy^2}\, dy = -m V^2 \frac{24\,D}{5a^4} \int \left(\tfrac{1}{2} y^2 - a y\right) dy,$$

integrating between limits $y = 0$ and $y = 2a$.

From this, it appears that the total centrifugal pressure

$$= \frac{32}{m} \frac{D}{V^2}\, 10\ a, \text{ which becomes } m g a \frac{1}{10} \frac{V^2}{a^2} D, \text{ if } g = 32$$

Now, if T be the number of seconds in which either end of the load traverses the beam, $\frac{V}{T} = 2a$, and $\frac{V^2}{a^2} = \frac{4}{T}$. Substituting this value, and remembering that the total weight on the beam is $2\, m g a$, we find ultimately,

$$\textit{Centrifugal pressure on whole beam} = \frac{1}{5} \frac{D}{T^2} \times \textit{ the weight.}$$

What very strongly confirms this conclusion, and shows that no materially great error is contained in it, is the consideration that if the curve had been supposed to be a *circular* arc passing through the middle and two ends of the curve, the effect of centrifugal pressure would be almost the same as the above formula gives it. The only difference (as will be seen hereafter) would be, that in the formula we must substitute $\frac{1}{4}$ for $\frac{1}{5}$. When it is considered how exceedingly small the curvature of the beam must necessarily be in all practical cases, it becomes clear that a circular arc of large radius would represent the curve with at least tolerable accuracy. At all events, that assumption furnishes a safe test of the foregoing conclusions.

Rule for Calculating the Pressure.

The formula then gives all the information that can be generally required, respecting the influence of the velocity of the train, or its pressure on the deflected girder, when the mass of the former is not small compared with that of the latter. Put into ordinary language, the formula amounts to this—that *when a long uniform load moves over a girder which is perfectly elastic, originally horizontal, the greatest pressure on the girder is that of the weight on it at any time + a small fraction of that weight, which fraction is found by dividing one-fifth the deflection (in parts of a foot) by the square of the number of seconds in which either end of the load traverses the girder.*

In order to give a clear idea of the value of the formula, and to show how small the influence of velocity generally is, one or two practical applications may be given.

A heavy train moves over a girder 88 feet long, at the rate of a mile a minute, and the observed deflection is one-third of a foot. To find the pressure on the girder.

In this case, either end of the train moves over the girder in one second. The square of the number of seconds is therefore 1. The deflection is $\frac{1}{3}$. Therefore, the fraction of the weight is $\frac{1}{15}$. Or the extreme pressure on the girder is one-fifteenth more than the weight on it at any time.

A train moves off a girder in three-fourths of a second, and the observed deflection is one-fourth of a foot.

Here the square of $\frac{3}{4}$ is $\frac{9}{16}$. One-fifth the deflection is $\frac{1}{20}$; and $\frac{1}{20}$ divided by $\frac{9}{16}$ gives $\frac{4}{45}$; or the pressure is not quite one-twelfth more than the weight at any time on the girder.

These instances give the dynamical pressure as large as it is ever likely to be with a properly-constructed girder-bridge. They consequently show that the dynamical pressure of heavy loads, even at high velocities, very little exceeds the statical; and at low velocities, differs from it only in an inappreciable degree.

It will be observed, that if the velocity be indefinitely increased, or T in the formula indefinitely diminished, the dynamical pressure is indefinitely increased. But the formula virtually excludes these hypothetical cases; for the investigation proceeded on the assumption that the centrifugal pressures are comparatively small, and that the whole pressure produces but a small deflection.

Defect of Elasticity.

It now remains that something be said of the defect of elasticity, and the modifications of the above results when applied to jointed or compound structures. The ordinary mathematical theory of the girder is based on the law of perfect elasticity, known as Hooke's law—namely, that the elastic force is proportional to the extension or compression.

It appears from experimental inquiries, subsequent to, and more extensive than, Dr. Hooke's, that this law is not quite true. The elastic force is in reality less than the law would assign it to be. Mr. Hodgkinson, in his recently published "Researches on Cast-Iron," Art. 106, seems inclined to think that the elastic force may be expressed by $a x - b x^2$, where x is the measure of compression or extension, and a and b constant empirical co-efficients. That this hypothesis is near the truth may be inferred from the consideration, that if the elastic force be expressed by a series in ascending powers of x, all terms involving high powers must be very small, as the elastic force is always nearly equal to the first term, and x is very small. It may, however, be worth while to remark, that if $a x - b x^2$ be taken to express correctly the elastic force, the same value of x, which reckoned positively gives the tension, will not, when reckoned negatively, give the same value for the pressure. In order that this may be the case, only uneven powers of x must be involved.

But whatever law of elasticity be assumed, this is easily ascertained—that where the elasticity is imperfect (that is, where it is less than in proportion to the extension or compression), the deflecting pressure of a girder will be less than in proportion to the deflection. In cast-iron girders, cast in one piece and in metal of good quality, the defect of elasticity is small; and consequently the deflection is pretty nearly proportional to the pressure. But in jointed structures, compounded of several parts connected by rivets or bolts, this is by no means the case. In them, the defect of elasticity must be great; and the deflection will therefore increase at a considerably higher rate than in proportion to the external pressure. If a load of 200 tons produce in a compound girder 2 inches deflection, 300 tons will produce considerably more than 3 inches deflection. Or, if 300 tons produce 3 inches deflection, 400 tons will produce considerably more than 4 inches deflection. *How* much more can be ascertained only from actual experiment.

It is very important that this distinction between simple and compound girders should be always taken into consideration, for the neglect of it would lead to very erroneous conclusions respecting the strength of structures of the latter kind. As cases in point, may be instanced calculations respecting the strength of girders formed in three pieces and supported by tension-rods. Formulæ which determine the strength of simple, unjointed girders, are inapplicable to these structures, and are not likely to give even an approximation to the amount of their real strength.

Where, however, the compound-girder is so well constructed, that its curvature, when deflected, is regular and free from sudden inflections, the formula given above for the dynamical pressure of long trains on perfectly elastic beams, will apply with considerable accuracy. For the deflection being previously assigned, is a safeguard against any very great error. That deflection being small, the curvature will also be small; only, on account of the defect of elasticity, it increases *cæteris paribus* more rapidly towards the centre of the beam, than it would if the beam were perfectly elastic. Consequently, the pressure towards the centre is comparatively greater in the compound, than in the simple girder: and pressure towards the centre is more effectual in producing deflection than pressure near the ends of the girder.

Consequently, there are two reasons why velocity increases the deflection of a compound, more than that of a simple, girder. In the first place, on account of the defect of elasticity in the jointed structure, its deflection increases in a higher degree than in proportion to the external pressure. Secondly, that external pressure is of necessity greater for the jointed than for the simple girder, because in the former the curve is sharper towards its centre. Velocity of transit has therefore much greater influence on the security of girders of the former, than of the latter kind.

It would have been satisfactory to have been able to confirm the results of these investigations, by reference to actual experiments. Unfortunately, however, there are at present but very scanty data for the purpose. An account of two experiments made on the Dee-bridge, of the Holyhead Railway, is all that can be cited. These experiments are described in a Report to the Commissioners of Railways, 15th June, 1847. An engine and tender, about 30 tons weight, passing over the bridge at 15 miles an hour, produced "a deflection nearly the same as with the engine at rest—viz., from $\frac{7}{8}$ to $1\frac{1}{16}$ of an inch." In another experiment, "an engine and train of 48 tons, at rest, gave a deflection of 2·4 inches; while the deflection caused by the same train at a speed of 15 to 20 miles an hour, was only $1\frac{3}{8}$ of an inch."

These accounts do not however furnish much information suited to our present purpose. In the first place, the experiments were made on a jointed structure of a complex nature, and of which the deflection appears, even from this brief account, to have followed no simple law. Moreover, in the first experiment, the deflection is not actually determined: it is merely said to have been from $\frac{7}{8}$ of an inch to three-sixteenths more; and in both, the mass of the girder greatly exceeded the mass sustained. All the inference that can be drawn is, that velocity did not very materially influence the deflection, but that the deflection was diminished at the highest velocity, the load sustained being comparatively light.

Means of Diminishing the Dynamical Pressure.

When a ball moves along a perfectly horizontal surface, the pressure on its under side is just equal to its weight, for this simple reason—that if the pressure were greater, the ball would rise; if less, sink.

In the same way, if a train moved along the surface of a girder which remained perfectly horizontal during the transit, its pressure would be just equal to its weight. But the train generally sinks a little, and acquires a momentum downwards, which has to be destroyed by increased pressure. The simplest precaution against this effect is—not to remedy it—but to prevent its existence. Suppose it be found that, when a certain weight travels along a certain girder which is originally perfectly horizontal, it produces a deflection of three inches at its centre: then, if the rails had a rise given them of three inches towards the centre, it is clear, that when the same weight travelled over them, it would be no lower when at the centre, than when at either end, of the beam.

Suppose now the reverse case—that there is a hollow or depression originally in the beam. Then, when the weight passes over the beam, it sinks the distance of this original depression, in addition to the deflection produced by pressure. Hence, the downward momentum is materially greater than if the beam had been perfectly horizontal originally. Or, to take another view of the question, the original hollow or depression, added to the deflection, increases the curvature of the beam, and therefore the centrifugal pressure of the load. Either way, then, of viewing the effect of the hollow, either in increasing the momentum downwards, or in increasing the centrifugal pressure, leads to the same result—that the pressure is increased. Mathematically, these two views of the case coincide.

It is seen, then, how extremely important it is that there should be no original hollow in the beam. On the contrary, it is advantageous that its surface should be convex, instead of concave—or should have a *camber*. In this case, the centrifugal pressure would act upwards instead of downwards; so that the pressure, instead of being greater than the weight, would be less at high speeds.

There is a very simple way of calculating this diminution, or of estimating the centrifugal force. And it may be remarked, parenthetically, that the method about to be given is useful for many purposes besides that to which we are to apply it. For example, it furnishes most simple and ready means of ascertaining the horizontal pressure on the flanges of the wheels of carriages going round railway curves.

If a be the length of the chord of a circular arc, which is of large radius, and x its lineal versed-sine, or the length of the perpendicular drawn from the centre of the arc to the chord, it will be found that the radius nearly equals $\frac{a^2}{8x}$.

Now, if V be the velocity of a mass m, moving round this curve, its centrifugal force becomes $m V^2 \frac{8x}{a^2}$; and if T be the number of seconds in which any part of the mass describes the distance a, $\frac{V^2}{a^2} = \frac{1}{T^2}$. Substituting this value, and putting $g = 32$, the centrifugal force is equal to $\frac{x}{4T^2} \times$ the weight. This formula applies to horizontal railway curves, as well as to the vertical curves of a beam. Confining attention however to this application of it, we see that a very considerable reduction of the pressure of the weight may be effected by curving the upper surface of the beam. Suppose, for instance, that the time of transit were one second ($T = 1$), and that it were practicable to give the rails such a convexity that the rise at the centre was one foot ($x = 1$). Then, from the formula, it appears that the centrifugal pressure would be one-fourth the weight, or that only three-fourths the weight pressed on the beam.

It may be remarked that this law has most important effects on such stupendous structures as the tubular bridges for the Chester and Holyhead Railway. The Conway-bridge, after it was constructed, sank eight inches at the centre by its own weight; this depression was anticipated and corrected by a previous upward convexity of the tube. But in this, and all analogous cases, a rise or convexity, considerably *exceeding* the natural depression, would tend greatly to security: because, the curve being convex, an increase of the velocity would diminish, instead of increasing, the pressure of a given load. It may therefore be safely asserted, that *it contributes to the security of girders to give their upper surfaces as great a convexity as is consistent with other practical requirements.*

General Conclusions.

At the close of these investigations, it may be convenient to recapitulate the general conclusions derived from them. They comprehend the following laws for the motion of very heavy loads at practical velocities over horizontal girders.

1. If the girder be perfectly elastic, the pressure exceeds the weight on the girder by a fraction of the weight, not more than one-fifth the actual deflection (in parts of a foot), divided by the square of the number of seconds in which either end of the load traverses the girder.
2. In compound and imperfectly elastic girders this fraction is increased.
3. The influence of the inertia of the beam on its deflection is inconsiderable.
4. In all girders, a convexity of their upper surface, or rise of the rails from end to centre, may be made to materially diminish the pressure.

These conclusions will, it is believed, furnish a tolerably accurate idea of the influence of moving loads upon railway girders. The only subject on which no definite investigation has been here attempted, is the defect of elasticity in jointed girders. The modes of construction of compound girders are so numerous, that to establish any general law respecting them is obviously impossible—no accurate knowledge can be derived of the law of elasticity or deflection in these cases, but by direct experiment.

No pains have been spared to render the views here expressed, correct. They have occupied many months of reflection, and have been subjected to the careful revision of the author's mathematical friends. As the great object has been to exclude all operose mathematical investigations, it will be readily understood that the subject, by constant corrections and simplifications, has assumed an entirely different shape to that originally given to it.

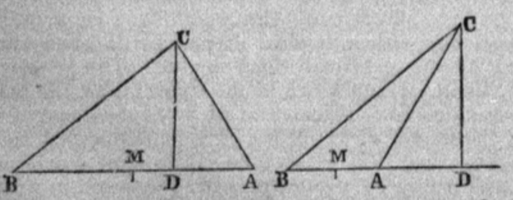
ON MR. CLARKE'S SURVEYING PROBLEM.

The problem proposed and solved by Mr. Clarke, in last month's *Journal*, p. 230, is by no means so new as he appears to think. The form under which the problem is most usually presented, is:—

Given the base and the angles at the base; to find the perpendicular, and the segments into which it divides the base.

Mr. Clarke's angles β and θ are the complements of the angles at the base of the triangle C A D, as is obvious. (*See* his figure, p. 230.)

The following investigation of the question is taken, almost literally, from the 12th edition of "Hutton's Course"; and it will be at once perceived to be more brief and simple than Mr. Clarke's.



Let C D be the perpendicular from the vertex to the base, and denote the angles of the triangle, as usual, by A, B, C, respectively. Then, by right-angled triangles, we have—

$$\mathrm{C\,D} \cot \mathrm{B} \pm \mathrm{C\,D} \cot \mathrm{A} = c, \text{ or}$$

$$\mathrm{C\,D} = \frac{c}{\cot \mathrm{B} \pm \cot \mathrm{A}} = \frac{c \sin \mathrm{A} \sin \mathrm{B}}{\sin(\mathrm{A} \pm \mathrm{B})} = \frac{c \sin \mathrm{A} \sin \mathrm{B}}{\sin \mathrm{C}}.$$

This value of C D is often required in problems of this class, giving (in Mr. Clarke's illustrative example) the *horizontal* distance of the point of observation from the observed object. It likewise as frequently occurs in determining the height of an object, as a hill, upon a horizontal plane.

From substituting the above value of C D in the equations,

$$\mathrm{A\,D} = \mathrm{C\,D} \cot \mathrm{A}, \text{ and } \mathrm{B\,D} = \mathrm{C\,D} \cot \mathrm{B}, \text{ we obtain}$$

$$\mathrm{A\,D} = \frac{c \cos \mathrm{A} \sin \mathrm{B}}{\sin \mathrm{C}}, \text{ and } \mathrm{B\,D} = \frac{c \sin \mathrm{A} \cos \mathrm{B}}{\sin \mathrm{C}}.$$

I have left these expressions in sines and cosines, instead of changing the denominator into the factor cosec C. There is no doubt that the better form of working, *when* C *does not contain seconds* (with the ordinary tables I mean, for surveyors seldom use tables to seconds), is the form which Mr. Clarke has adopted: but in the other case it is somewhat questionable.

As, however, this is a mere question of experience—perhaps, too, of habit—every one should adopt the plan he can most easily use.

This mode of treating such problems is, in fact, the same with finding the co-ordinates of the point of observation, referred to the horizon and the vertical object observed. I have often been led to think, that if the greater part of the problems (if not all) which occur in surveying were systematically treated, according to the calculus appropriate to the co-ordinate system, the processes of computation would be considerably improved. Even were the actual work not materially lessened, the *systematising* of the entire class of problems would be in itself a great practical advantage.

When, however, we confine ourselves, as Mr. Clarke has done, to finding the difference of levels, a still shorter method of operating may be used, for it requires one reference less to the tables. It may be thus investigated.

Let M be the middle of A B; denote A B by $2a$, and M D by x.

Then (fig. 2) A D $= x - a$, and B D $= x + a$; whence

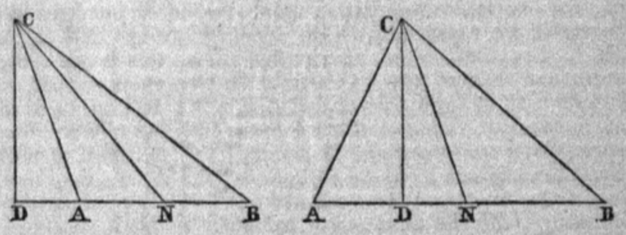
$$\mathrm{A\,D} \tan \mathrm{A} = \mathrm{C\,D} = \mathrm{B\,D} \tan \mathrm{B}, \text{ becomes}$$

$$(x - a) \tan \mathrm{A} = (x + a) \tan \mathrm{B}, \text{ or}$$

$$\frac{x - a}{x + a} = \frac{\tan \mathrm{B}}{\tan \mathrm{A}}; \text{ or again, } x = \frac{a \sin(\mathrm{A} + \mathrm{B})}{\sin(\mathrm{A} - \mathrm{B})}.$$

A corresponding form, adapted to the case represented in fig. 1, is deducible in the same way; but further notice of it here is unnecessary. A formula so simple, and so easily derived, can scarcely be new. Still, I do not recollect to have noticed it elsewhere.

Another variation of the same general problem is often useful. It is, *where the segments of the base* A B, *and the angle* C, *are given, to find the perpendicular* C D.



Put D A $= a$; D B $= b$; A C B $= 2\gamma$; let C D bisect B C A; and denote N C D by θ, and C D by x. Then we have

$$\mathrm{B\,C\,D} = \theta + \gamma, \text{ and } \mathrm{A\,C\,D} = \pm(\theta - \gamma);$$

$$\text{and } a = \pm x \tan(\theta - \gamma);\ b = x \tan(\theta + \gamma).$$

Wherefore,

$$\frac{a}{b} = \frac{\pm \tan(\theta - \gamma)}{\tan(\theta + \gamma)} = \pm \frac{\sin(\theta - \gamma)\cos(\theta + \gamma)}{\cos(\theta + \gamma)\sin(\theta - \gamma)} = \pm \frac{\sin 2\theta - \sin 2\gamma}{\sin 2\theta + \sin 2\gamma}$$

$$\text{and hence, } \sin 2\theta = \pm \frac{a - b}{a + b} \sin 2\gamma.$$

Whence the angle θ becomes known, and is very easily computed; and hence the perpendicular C D is obtained from either of the preceding equations,

$$x = \pm a \cot(\theta - \gamma),\ x = b \cot(\theta + \gamma).$$

—But I need not further dilate on so simple a subject.

UBIQUE.

5th August, 1848.

THEORY OF STEAM-ENGINES.

Account of the experiments to determine the principal laws and numerical data which enter into the calculation of Steam-Engines. By M. V. Regnault.

(Continued from page 240.*)*

Second Memoir.—THE DETERMINATION OF THE DENSITY OF GASES.

The common method of determining the density of a gas consists in weighing a glass globe of great size:—

1st. When the globe contains perfectly dry air of a known temperature, and under a known atmospheric pressure:

2nd. After having exhausted it by means of the air-pump, so that the inclosed air exerts only a very feeble pressure, which, as well as the corresponding temperature, is noted:

3rd. After having filled the globe with the gas perfectly purified, the atmospheric pressure and temperature being again noted:

4th. After having again exhausted the globe, noting, as before, the pressure and temperature.

By which four weighings, and their accompanying observations of pressures and temperatures, all the data necessary for determining the density of the gas are given. But this method requires the exact knowledge of several elements, the determinations of which present generally great uncertainty.

In the first place, the temperatures which the air and the gas present at the moments of closing the globe, must be very exactly known; but the method generally used of having a thermometer placed near the globe is very defective: the temperature indicated by the thermometer may be totally different from that of the air in which it is bathed, and still more so from that of the gas which fills the globe.

MM. Dumas and Boussingault, who have lately (*Ann. de Chim. et de Phys., 3d série, tome* III., *p.* 270) very successfully endeavoured to determine the densities of certain gases, place the thermometer intended to indicate the temperatures of the gases in the very centre of the globe; and for more security, they cause that temperature to be almost constant, by placing the globe in an inclosure formed by a large cylindrical vessel of zinc with double walls. The annular space left between the walls is filled with water, at a temperature differing but little from that of the surrounding air. With this arrangement, we may admit that the temperature of the gas is known with sufficient accuracy.

But the greatest uncertainties exist in the weighings of the globe; for we must weigh the globe in the air, and, to have its true weight, we must add to its apparent weight that of the air which it displaces. And, in certain cases, this latter weight is greater than that of the gas which fills the globe, so that it must be known with at least equal exactness. We are, up to a certain point, masters of the gas which we pass into the globe. We may prepare it so as to be sure of its purity; but it is not so with the external atmosphere; we are obliged to take it as it is. In a close chamber the air may change its composition very perceptibly; its temperature, and the quantity of moisture which it contains, very incessantly. MM. Dumas and Boussingault thought that they had completely avoided the errors arising from this source, by placing below their balance a large chest lined with lead, in which the globe suspended from one of the scales of the balance floats. A very sensitive thermometer is placed in this chest, and gives the temperature of the air. This arrangement is certainly far preferable to allowing the globe to hang freely in the air of the room; the globe suspended in the chest is kept from the currents of air, which render the weighings very uncertain, and the temperature of the air in which it is placed changes but slowly; but it does not do away with the errors arising from the changes in the composition of the air, and these are by no means negligible, especially when we are working with very light gases—such, for instance, as hydrogen.

At the same time that MM. Dumas and Boussingault were weighing gases, M. Regnault was also engaged in the determination of the density of steam under different pressures, and especially under very feeble pressures. He was struck with the uncertainties which the ordinary methods of weighing gases present, especially owing to the alteration of the density of the surrounding air, which seems to have attracted but little attention from men of science, and he was led to a method which presents a degree of certainty and precision which those heretofore used do not offer.

He avoids completely, and by a very simple artifice, the uncertainties which arise from the changes in the air in which the globe is weighed. In place of equipoising it by means of weights on the opposite scale, he balances it by means of a second hermetically-sealed globe of the same kind of glass, hung from the opposite scale. All the variations which take place in the air then affect the two globes in the same way, whether they arise from changes of temperature, barometric pressure, or composition of the atmosphere. It is not, therefore, required at the moment of weighing to watch the thermometer, barometer, and hydrometer; it is sufficient to wait until the two globes are in equilibrium of temperature, and when this is the case, it continues indefinitely. We have, in consequence, a very definite character by which to know when to read the weighings. This method presents also another advantage, that is, of avoiding the error arising from the different amount of moisture deposited upon the globe during different weighings. As the globes are made of the same glass, and equally dried before the commencement of the experiments, they may be assumed to condense the same amount of moisture when bathed in the same air, and consequently will remain in equilibrium.

The globes had a capacity of about 10 litres (2·2 gallons). The globe in which the gas is to be weighed has a stop-cock adjusted to it, so that it may be exposed to the temperature of boiling water without leakage. It is weighed when full of water, first in the air, and afterwards in water of the same temperature as that which it contains; thus is obtained the weight of water displaced by the globe.

The balancing globe is so selected that the weight of water displaced by it is rather less than that of the first, even after the addition of its metallic mounting by which it is hermetically sealed, and hung from the opposite balance scale; there is then added to it a glass tube of such capacity as that the weight of water displaced by it shall just make up the difference.

Before closing the second globe, a quantity of mercury was introduced into it so as to render it about 10 grammes (154·38 grains troy) heavier than the other. The two globes thus adjusted were submitted to several tests, in order to be sure that they satisfied the required conditions; they were left hanging for fifteen days under the balance scales, and the equilibrium was rigorously maintained all this time, although in the interval the temperature of the air had changed from 0° to 17°, (32° to 62·5° Fahr.,) and the barometric pressure from 741 to 771 millimetres (29·6 to 30·8 inches.) The general mode of operating was as follows:—A vacuum as complete as possible being made in the globe, it is placed in communication with the apparatus for producing the gas whose density is to be determined, and the stop-cock is opened in such a way that the gas in the apparatus preserves a slight excess of pressure. When the globe is filled with gas, it is again placed in communication with the air-pump, a very perfect vacuum made, and it filled a second time with the gas. In order to avoid any correction for temperature—a correction which would require the knowledge of the co-efficient of dilatation of the gas, and that of the globe—the globe is placed in a zinc cover and completely enveloped in melting ice. Before closing the globe, it is placed in direct communication with the atmosphere, so that the gas may place itself in equilibrium with the exterior pressure.

The globe taken out of the ice is carefully washed and dried, and hung from the scale of the balance. It requires a long time (often more than two hours) for the globe to take exactly the temperature of the surrounding air, and for its surface to cover itself with its normal amount of moisture. The balance used was able to appreciate with certainty a half milligramme when charged with one killogramme upon each scale.* It was placed over a large chest, such as used by MM. Dumas and Boussingault. At the end of the weighing, the observer did not approach the balance, but observed the oscillations of the index at a distance, with a telescope.

M. Regnault then notices the electric effects produced by wiping the globes, and the effects upon the weighings; he avoided it by wiping the globes with a napkin dipped in distilled water, and tested them by the gold-leaf electrometer. The pressures were measured by an apparatus which he describes under the name of a barometric-manometer. It consists of two tubes, one of which is an ordinary barometer of 20 millim. (0·8 in. diameter,) made very carefully; the other is a glass tube of the same diameter, which may, by a lead tube, be connected with the vessel in which pressures less than that of an atmosphere are to be measured; they plunge below into a cistern of mercury having a partition, so that the two instruments may be separated at pleasure by drawing off the mercury in the cistern of the manometer below the top of the partition; this is necessary during the exhaustion of the globe, and the re-admission of the gas, for these operations produce such great oscillations in the barometer as to introduce small quantities of air into the instrument, and thus vitiate the vacuum. The exhausted globe is weighed with the precautions that have been indicated. If P represent the weight of the gas when the barometer stands, at H, and p that weight when the elastic force in the globe corresponds to a barometric height h, the weight of the gas at 0°, and under the normal pressure of 760 mil., is $(P-p)\frac{760}{H-h}$

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* That is, sensible to one four-millionth of the total load. Quere, whether this sensibility was determined after the globes had been hanging from the scales, and the balance in action for fifteen days?

To obtain a new weighing of the same gas, the exhausted globe, enveloped in ice, is placed in connection with the apparatus for generating the gas, and the series of operations which have been pointed out, repeated. The gas thus becomes purer at each operation. M. Regnault found that it is only from the fourth filling that the gas presents rigorously the same weight. It is desirable to satisfy one's self whether the gas upon which we are operating follows the law of Mariotte, at pressures below those of the atmosphere: this verification is absolutely necessary if the density of the gas is to serve for the determination of atomic weights. For the law of the volumes of gases, and the simple ratios which exist between their densities and atomic weights, exist rigorously only *at the limit*—that is, in a state of extreme dilatation; we must therefore see whether the anomoly in these laws does not commence already near the atmospheric pressure.

This is done by measuring the weight of the gas, with great care, at different degrees of elastic force, as marked by the comparison of the manometer and barometer.

Finally, by this means we may determine the weight of the gas which fills the globe at the temperature of 100° and under atmospheric pressure, and thus determine the density of the gas when compared with air at 100°. This new density must be exactly the same as that calculated for 0°, in order that it may serve in the calculation of the atomic weights; for it is necessary for this purpose that the gas should have the same co-efficient of dilatation as the atmospheric air; at all events the weight of the gas which fills the vessel at 100° compared with that which fills it at 0°, permits us to calculate the *co-efficient of the dilatation of the gas.**

Again, in order to determine whether the gas follows the law of Mariotte, at the temperature of 100°, we have only to repeat the former experiments, filling the globe at this temperature, instead of at 0°.

M. Regnault then recapitulates the advantages of this method, which are,—that it gives the density of the gases with more precision, and far less trouble, than the methods formerly used; it gives these densities at identical temperatures at 0° and 100°, that is, at the fixed points of the thermometer, and consequently gives immediately the co-efficient of dilatation of the gas; and, finally, it permits us to determine with great exactness, whether the gas follows the law of Mariotte, at the temperatures of melting ice, and boiling water.

He then proceeds to give the detail of all his experiments, without a single exception, in order to allow the reader to judge of the degree of precision obtained by this method. It is not necessary that we should give these details—or those of the processes by which M. Regnault purified his gases; they were such as might be expected from one so familiar with all the minutiæ of physical science.

He first determined by nine experiments the weight of pure atmospheric air, freed from carbonic acid and watery vapour, which filled his globe at the temperature of 0°, and under the barometric pressure of 760 millim. (29·944 inches). The mean of these experiments was 12·7781 gr. The minimum, 12·7744. The maximum, 12·7809. The difference, 0·0065 or $\frac{1}{1977}$, very nearly $\frac{1}{2000}$ of the mean; and he remarks that it is probable that a great part of this error is due to the variations which occur in the composition of the atmosphere. He regards it as unfortunate that men of science should have selected the atmospheric air, whose constitution is known to vary, as the standard of densities for gases, in place of some gas which could always be obtained perfectly pure, such for instance as oxygen, which would be the more convenient since this gas is already chosen as the basis of the tables (adopted by continental chemists) of chemical equivalents.

Founded upon this determination of the weight of a given volume of air, he proceeds to determine the densities of different gases, and his results are as follows:—

	Tem. at 0° Bar. 760 m.	Minimum.	Maximum.	Difference.
Nitrogen, mean of 6 Experiments,	0·97137	0·97108	0·97155	$\frac{1}{2000}$
Hydrogen, „ 3 „	0·06926	0·06923	0·06932	$\frac{1}{770}$
Oxygen, „ 3 „	1·10563	1·10561	1·10565	$\frac{1}{8000}$
Carbonic Acid, 5 „	1·52910	1·52900	1·52915	$\frac{1}{1020}$
„ at 0° and 374·13 millim.	1·52366			
„ „ 224·17 „	1·52145			
„ 100° 760 00 „	1·52418			
„ „ 383·39 „	1 52410			

Densities determined by Dumas and Boussingault.

Nitrogen, from	..	0·970	to	0 974
Hydrogen, „	..	0·0691	„	0·0695
Oxygen, „	..	1·1055	„	1·0158

If we calculate the theoretic density of carbonic acid gas, admitting for the atomic weight of carbon 75, (oxygen = 100, or 6 if hydrogen = 1,) lately found by M. Dumas, we get the number 1·52024, which approaches the density found for this gas under the pressure of 224·17 millim. (less than nine inches.)

The density found at the temperature 0° and normal atmospheric pressure leads to an atomic weight for carbon 76·6 which approaches very nearly the number 76·44 (6·1152, if hydrogen = 1) which chemists for a long time admitted from the experiments of M. Berzelius.*

We see, by this example, how much circumspection is necessary in deducing the value of the atomic weight of a gas from its density.

Three experiments to determine the co-efficient of dilatation of the air between 0° and 100°, gave as the result 0·03663, which differs but little from the value obtained in the First Memoir.

An attempt to verify the law of Mariotte showed slight differences, in which the weights by experiments were always a little lower than those got by calculating the density by means of Mariotte's law from the observed elastic force; but these differences were always within the limits of the errors of observation.

The co-efficient of dilatation of carbonic acid gas between 0° and 100° was determined to be 0·003719. (In the First Memoir, the determination by the method V., in which the gas preserved the same elastic force at 0° and 100°, as in the present case, was 0·0037099.)

The experiments to determine whether carbonic acid gas obeys the law of Mariotte at pressures less than that of the atmosphere, gave the following results:—

				By experiment.	Calculated by Mariotte's law.
Weight of the gas at	0°,	and with an elastic force	224·17 mil.,	5·7345 gr.,	5·7634 gr.
„	„	„	374·13 „	9·5845 „	9·6628 „
„	101·01°	„	338·39 „	6·3549 „	6·3545 „

so that it appears that carbonic acid gas deviates notably from the law of Mariotte at ordinary temperatures, but conforms to it with the limits of experimental errors at 100°.

THIRD MEMOIR.—DETERMINATION OF THE WEIGHT OF THE LITRE† OF AIR AND OF THE DENSITY OF MERCURY.

In the preceding memoir, the densities of the different gases were determined, referring them to that of air assumed as the unit; but in a great number of circumstances, it is required to know the absolute weight of these gases: this is easily obtained when we know the absolute weight of air under the normal conditions, that is at a temperature of 0°, and under a pressure of 760 millimetres of mercury.

The weight of the litre of dry air under the normal conditions was determined by MM. Biot and Arago, with all the care which they could take—they found that at Paris this weight was 1·299541 gr. (*Memoirs of the Academy of Sciences for* 1806. *Biot Traité de Physique, tom.* 1, *p.* 387.) This number has been generally adopted.

But if we reflect upon the imperfections which the theory of gases and vapours still presented at that time, and the great number of uncertain corrections which they were obliged to introduce into their calculations; and if we note that they operated upon air charged with aqueous vapour, for which they endeavoured to allow by a correction; and that, in spite of the most minute precautions, this circumstance must necessarily introduce great disturbances into their experiments, we shall understand how absolutely necessary it was to make new determinations of this important doctrine, which will be frequently used in the following investigations.

In the preceding memoir, the weight of dry air which filled the globe at 0°, and under a pressure of 760 mil., was determined with great care; it will be enough then to find the capacity of this globe at 0°, to determine immediately the weight of the litre of air. Now, according to the principle upon which the French system of measures was established, the kilogramme is the weight of a litre of distilled water, freed from air, at the temperature of its maximum density, which is about 4° (39·2 Fahr.); it will suffice then to determine the weight of water at 4° which fills the capacity which the globe presents at 0°.

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* This is the only direct method which can be used for this determination, in gases which attack mercury.

* Berzelius himself now acknowledges the error of his former determination, and fixes the atomic weight of carbon at 75·12 or 6·01 (H = 1), which amounts to an admission of Dumas's determination. (Berzelius Traité de Chimie, Seconde Edition Française, Paris, 1845, tom. 1, p. 263.)

† The French litre is equal to 0·22 of the imperial gallon.

To do this M. Regnault operated in the following way:—

The open globe was weighed upon a good balance; its weight was found to be 1258·55 gr., the surrounding temperature being 4·2°, and the height of the barometer reduced to 0°, 757·89 mil.

A small quantity of water was introduced into the globe, and the globe exhausted by means of the air-pump, and at the same time heated. In this way the atmospheric air was completely expelled by means of the vapour of water which was constantly developed. The stop-cock of the globe was then closed.

On the other hand, perfectly pure distilled water was boiled in a large globe to free it completely from the air which it always holds in solution at ordinary temperatures. Upon the tubulure of the first globe was fixed by caoutchouc a glass tube, twice bent, one of whose branches descended to the bottom of the vessel in which the water was kept boiling. On opening the stop-cock of the globe, the boiling water entered it slowly, without coming in contact with air; it was consequently perfectly free from that gas.

The globe being completely filled, the recurved tube was removed, and replaced by a tube having a bulb which was kept filled with the boiling water, and furnished the quantity of water necessary to keep the globe filled as its temperature lowered.

When the globe, filled with water, had come down to the surrounding temperature, it was placed in a zinc vessel, and completely surrounded with melting ice, care being taken to pack the ice in proportion as it melted, upon the walls of the globe.

The globe was left in the ice for a time varying from 6 to 18 hours; the stop-cock was then closed, the bulbed tube detached, and the tubulure above the stop-cock carefully wiped.

The globe was placed in a large vessel filled with water at a temperature a little above that of the chamber in which the balance was; it was left for two hours, so that it should take nearly the temperature of the chamber. As the water contracts in proportion as its temperature rises from 0° (to 4°), the globe could be kept closed without danger of breaking. When the globe had acquired the temperature of the chamber, it was weighed, and this weighing (the temperature of the room and the height of the barometer being noted) gives the means of calculating the weight of water at 4°, which fills the capacity which the globe presents at 0°.

According to the experiments of M. Pierre (*Annales de Chimie et de Physique, 3d série, tome* xv., *p.* 348), if the density of water at 0° be taken as 1, at 4° it is $\frac{1}{0{\cdot}999881}$.

Whence we can calculate the weight of the water at 4° (its maximum density), which fills the capacity which the globe presents at 0°. Three experiments give the following results:—

I. 9881·060 grammes.
II. 9881·113 „
III. 9881·299 „

The third weighing gave a number probably a little too high, because the globe was intentionally left but a little time in the ice, in order to see what influence this circumstance would have upon the result. On this account, M. Regnault adopts the mean of the former experiments, viz.: 9881·086.

Desiring to ascertain whether the correction made to reduce the weight of water from 0° to 4° was sufficiently exact, M. Regnault made two direct experiments, which gave a mean differing only 0·152 or $\frac{16}{100,000}$ from the result of the calculation. The capacity of the globe at 0° was therefore 9·881086 lit., and since (*see* Second Memoir) the weight of air which filled it at 0°, under a pressure of 760 mil., was 12·7781 gr.; the weight of the litre of air, under these normal circumstances is $\frac{12{\cdot}7781}{9{\cdot}881086}$ gr. $=$ 1·293187 gr. a value notably less than that which was heretofore admitted from the experiments of MM. Biot and Arago[*] (1·299541).

From this and the numbers obtained in the preceding memoir for the densities of the gases we deduce, that at Paris

The litre of	Atmospheric Air	weighs	1·293187	grammes.
„ „	Nitrogen	„	1·256167	„
„ „	Oxygen	„	1·429802	„
„ „	Hydrogen	„	0·089578	„
„ „	Carbonic Acid	„	1·977414	„

Strictly considered, these values are only correct for the locality in which the experiments were made—that is, for a latitude of 48° 50′ 14″, and a height of about 60 metres above the level of the sea.

M. Regnault finds the weight of the litre of air, under the parallel of latitude 45°, and at the same distance from the centre of the earth as that at which his experiments were tried, = 1·292697.

And assuming this as the standard number, he deduces for any other latitude, and any other distance from the centre of the earth, the formula

$$w = 1{\cdot}292697 \text{ gr. } (1{\cdot}00001885) \frac{1}{1 + \frac{2h}{R}} (1 - 0{\cdot}002837, \cos 2\lambda).$$

Or, more simply

$$w = 1{\cdot}292673 \text{ gr. } \frac{1}{1 + \frac{2h}{R}} (1 - 0{\cdot}002837, \cos 2\lambda);$$

in which w is the weight of the litre of air (the litre is 61·09908 cubic inches); R, the mean radius of the earth = 6,366,198 metres; h, the height of the place of observation above this mean radius, expressed in metres; and λ, the latitude of the place.

Applying this formula to the level of the sea, in the latitude of Philadelphia (39° 56′ 51·5″), and assuming the radius of the earth at this point 6,367,653 metres:

The weight of the litre of air will be 1·2914392 grammes.

And assuming the litre as 61·09908 cubic inches, and the gramme as 15·433159 grains troy:

The weight of a cubic inch of air will be 0·32621 grains troy.

Or, (assuming Mr. Hassler's determination of the weight of a cubic inch of water, 252·6934 gr.) water is 774·63 times heavier than air.

Density of Mercury.

The density of mercury has been determined several times by M. Regnault, and with the greatest care; as he wished to satisfy himself whether this liquid, purified by the means employed ordinarily in the laboratories, presented a constant density.

A glass globe, of a capacity of from 250 to 300 cubic centimetres, was filled with mercury. The globe terminated in a capillary tube of about 2 mil. diameter, upon which a mark was made, and this tube was surrounded by a larger one which was used as a funnel. The funnel could be hermetically closed by a ground glass stopper. The globe being filled with mercury, this liquid was boiled, and suffered to cool. The globe was then placed in ice for several hours, and the level of the mercury brought exactly to the mark. As soon as it was satisfactorily ascertained that the level of the mercury did not change, the mercury was suffered to take the temperature of the air, and its weight determined. The same globe was then filled with distilled water, first boiled to deprive it of air. It was suffered to cool, the funnel being kept full of boiled water, and closed with its stopper. The globe was then surrounded with ice, and when the water had taken exactly the temperature 0°, the water level was brought to the mark, and the sides of the funnel wiped with filtering paper. The closed globe was then placed in water having nearly the temperature of the surrounding air, so as to bring it more quickly to the temperature of the air in which it was to be weighed.

The three determinations of the density of mercury, which are reported, were made at very different times, upon specimens from different sources, and in three different globes:—

I. The first specimen was mercury designed for the construction of a standard barometer for the observatory of Paris. This mercury came directly from the mine; it had been twice distilled in an iron vessel. It was then suffered to stand for several days under weak nitric acid, to dissolve the oxide of mercury which always forms during distillation. The metal was then washed with much water, and dried in the air-pump. The density of the mercury at 0°, compared with that of water at 4°, was 13·59599.

II. In the second experiment, the mercury employed was that used by M. Regnault, in the construction of his manometer. This mercury was distilled several years ago, in an iron retort, and has been kept in glass vessels. It was frequently purified by shaking it in flasks with concentrated sulphuric acid, then washing with much water. Its density at 0°, compared with that of water at 4°, was 13·59578.

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[*] M. Regnault remarks, in a note, that all the numerical corrections made by MM Biot and Arago, for the purpose of reducing the weight of air to 0°, and to absolute dryness, contributed to render the number which they adopted too high. Another circumstance may have produced a similar effect. These experimenters exhausted the globe several times with a very good air-pump, and they supposed that the slight tension which remained in their globe was produced by the vapour of water which the walls of the globe abandoned in vacuo, which they re-condensed when the air entered again. It is, in fact, probable that this was the case; but it seems to be also very probable that when the globe was filled with air very nearly saturated with moisture, it gave a new portion of water to the glass. This portion, which was not taken into account, was considered as making a part of the weight of the air, and necessarily made that weight too great.—(See Biot's "Traité de Physique," tome 1, p. 367.)

III. Recently, M. Regnault has determined the density of mercury prepared with the greatest care by M. Millon, by the calcination of crystallized nitrate of mercury in a porcelain retort. The metal was then shaken up with concentrated sulphuric acid, to dissolve the oxide. Density of the mercury, 13·59602.

Thus we find for the densities of these three specimens of mercury :—

$$\left.\begin{array}{r}\text{I. } 13{\cdot}59599\\ \text{II. } 13{\cdot}59578\\ \text{III. } 13{\cdot}59602\end{array}\right\} 13{\cdot}59593.$$

These densities may be considered as identical.

MM. Biot and Arago found the density of mercury, 13·588595.

This density differs but little from those which we have found. The little difference ought probably to be attributed to the uncertainty of the corrections which these illustrious physical philosophers were obliged to make in their method of operating.

It is often necessary, as in measuring heights by the barometer, to know the ratio of the density of mercury to that of air.

Now 1 lit. of air at 0°, under a pressure of 760 mil., weighs 1·293187 gr.
„ water at its maximum density weighs 1000·000000 „
„ mercury at 0° „ 13595·93 „

The ratio of the densities of mercury and air at the temperature 0°, and under the pressure of 760 mil. observed at Paris, is then 10513·5. At the level of the sea, and in latitude 45°, it becomes 10517·3; and at the level of the sea, at Philadelphia, 10527·735.

(To be continued.)

CONTRIBUTIONS TO RAILWAY STATISTICS,

IN 1846, 1847, AND 1848.—By HYDE CLARKE, Esq.

(Continued from page 245.)

No. III.—COAL TRAFFIC.

Coal traffic is one of the largest and most important items of railway transit; but here, as elsewhere, the returns published by the Railway Department are insufficient to show the whole amount. This is the more to be regretted, as the great reduction in the price of coal by railways has largely increased the demand for household use, as well as for manufacturing purposes. The monopolies of the canal proprietors, and of the wharfingers connected with them, have been broken up, and each year some new operation throws open a fresh district.

Coal traffic is of three kinds: from the colliery inland; from the colliery to the sea; and from the sea-shore inland.

The following shows the gross tonnage of coals, coke, and culm on the undermentioned lines for the years ending 30th June, 1846 and 1847.

Name.	1846. Tons.	1847. Tons.
Arbroath and Forfar, ..	12,012	14,025
Ardrossan, ...	66,782	70,000†
Ballochney, ...	139,206	185,969
Bodmin and Wadebridge, ..	5,123	5,129
Caledonian (Glasgow and Garnkirk)	227,183	335,319
Dunfermline and Charlestown,	28,654	27,626
Dundee and Arbroath, ..	19,000	500†
Dundee and Newtyle, ..	10,000*	10,000
Durham and Sunderland, ..	394,974	——
Eastern Counties: Cambridge,	15,000*	15,000*
„ Colchester,	26,976	20,000*
„ Ipswich and Bury,	——	15,749†
„ Eastern Union,	——	18,744†
„ Norfolk,	6,000	15,000*
Edinburgh and Dalkeith,	95,571	——
East Lancashire, ..	——	1,181†
Furness, ...	——	2,748
Glasgow and Greenock, ..	28,429	30,387
Glasgow and Ayr, ..	180,130	242,443
Great North of England, ..	251,484	——
Hartlepool, ..	893,701	789,673
Hull and Selby, ..	40,000*	——
Kendal and Windermere,	——	100†
Lancaster and Carlisle, ..	——	6,886
Lancaster and Preston, ..	10,000*	——
Lancashire and Yorkshire	59,919	66,175
„ Preston and Wyre,	29,390	33,978
„ Manchester and Bolton,	49,255	27,469†
Llanelly and Llandilo, ..	97,017	84,150
London and North Western (Birmingh.)	37,889	440,000*
„ (Grand Junction)	323,905	——
„ (Manchester and Birm.)	119,774	——
London and Brighton: Brighton,	30,000*	40,000*
„ „ Croydon,	9,118	——
London and South Western, ..	18.830	31,659
Manchester and Bolton, ..	172,230	——
Manchester and Sheffield, ..	30,569	82,008
Maryport and Carlisle, ..	135,106	179 748
Midland, ...	305,904	370,787
„ (Bristol and Birmingham)	89,421	110,557
Middlesborough and Redcar, ..	——	4,894
Monkland and Kirkintilloch	400,000*	400,000*
Newcastle and Carlisle, ..	190,068	236,649
Newcastle and Darlington, ..	689,892	——
Newcastle and Berwick (North Shields)	29,345	33,866
North Union, ..	395,021	233,137
North British, ...	——	15,756
Preston and Longridge, ..	——	248†
Pontop and South Shields, ..	596,369	——
Slamannan	——	78,888
South Eastern, ...	45,350	71,723
Scottish Midland, ...	——	5,124
Stockton and Darlington, ..	904,358	911,645
Stockton and Hartlepool, ..	24,408	7,165
„ „ (Clarence)	497,100	537,333
Shrewsbury and Chester, ..	——	1,115
St. Helen's, ...	245,573	247,734
Taff Vale, ...	284,066	314,621
Ulster, ...	3,379	2,716
West Cornwall, (Hayle) ..	24,942	28,416
Wishaw and Coltness, ..	434,365	678,239
Wilsontown, ...	500	4,244
Whitehaven, ...	514	7,000†
York and North Midland, ..	159,608	185,982
York and Newcastle, ..	——	1,620,163

* Supposed amount. † Imperfect return.

The quantity enumerated amounts to about 8,900,000, or nearly 9,000,000; the number enumerated in 1845 being 7,000,000 tons.

The amounts received for the carriage of coals in the years ending June 30, 1846 and 1847, were as follows:—

Name.	1846. £.	1847. £.
Arbroath and Forfar, ..	1,749	2,012
Ardrossan, ...	2,339	3,200†
Ballochney, ...	3,765	7,091
Bodmin and Wadebridge, ..	616	628
Caledonian (Glasgow and Garnkirk)	5,123	17,535
Chester and Birkenhead, ..	——	120†
Cockermouth and Workington,	——	105†
Dunfermline and Charlestown,	3,078	2,910
Dundee and Arbroath, ..	4,000	28†
Durham and Sunderland, ..	20,604	——
Eastern Counties: Cambridge,	22,796	5,158
„ Colchester,	7,274	——
„ Eastern Union,	——	200†
„ Ipswich and Bury,	——	1,264
„ Norfolk,	700	2,000*
East Lancashire, ..	——	51†
Edinburgh and Dalkeith, ..	4,160	——
Glasgow and Greenock, ..	2,917	3,323
Great North of England, ..	20,978	——
Hartlepool, ..	35,958	31,477
Lancaster and Carlisle, ..	——	929†
Lancashire and Yorkshire, ..	2,257	4,840
„ Preston and Wyre,	3,172	2,821
„ Manchester and Bolton,	3,043	1,142
Llanelly and Llandilo, ..	6,579	5,284
London and North Western (Birmingham)	4,633	36,657
„ (Grand Junction)	17,807	——
„ (Manchester and Birm.)	5,470	——
London and Brighton (Brighton)	2,000*	——
„ „ (Croydon)	683	——
Manchester and Sheffield, ..	1,933	5,564
Maryport and Carlisle, ..	10,513	13,962
Midland, ...	63,183	56,590
„ Bristol and Birmingham,	8,668	7,660
Middlesborough and Redcar,	——	325
Newcastle and Carlisle, ..	18,259	23,944
Newcastle and Berwick (North Shields)	1,314	1,474
Newcastle and Darlington, ..	31,679	——

North Union,	20,000*	20,000*
North British,	——	1,571
Preston and Longridge,	——	16†
Pontop and South Shields,	46,283	——
Stockton and Darlington,	65,736	71,842
Stockton and Hartlepool,	782	247
„ „ (Clarence)	30,261	33,472
Slamannan,	——	2,325
South Eastern,	——	9,554
Scottish Midland,	——	349
Shrewsbury and Chester,	——	86
St. Helen's,	10,671	10,469
Taff Vale,	24,447	28,620
Ulster,	406	376
West Cornwall (Hayle)	3,220	3,667
Wishaw and Coltness,	13,623	19,184
Whitehaven,	14	350†
Wilsontown,	36	233†
York and Newcastle,	——	111,384
York and North Midland,	16,179	19,637

* Estimated amount. † Imperfect return.

This constitutes a total of nearly £550,000, so that the gross total is most probably nearly £700,000, being the sum received by railways on account of the conveyance of coal. Among the lines omitted are the following:—

Great Western,
Edinburgh and Glasgow,
Glasgow and Ayr,
London and South Western,
Eastern Counties: Colchester,
Monkland and Kirkintilloch.

The rates of charge vary much on the several lines, depending on many circumstances, so that it is impossible to institute an accurate comparison. In some cases, the coal-owners supply their own locomotives and wagons, and are charged with toll only. In others, they supply wagons only. In others, they are charged with an additional rent for wagons. In many cases the company hauls and supplies wagons.

Rate per ton per mile for toll only, and for total charges—

	Toll. *d.*	Total charges. *d.*
West Cornwall,	—	5·82
Dunfermline and Charlestown,	—	4·29
Bodmin and Wadebridge,	—	4·
Newcastle and Berwick (North Shields)	—	3·50
Arbroath and Forfar,	2	3·30
Cockermouth,	—	3·11
Scotch Midland,	—	3·07
Kendal and Windermere,	—	3·
Ballochney,	—	3·
Preston and Longridge,	—	2·66
Furness,	—	2·50
Maryport and Carlisle,	1·30	2·47
Londonderry and Enniskillen,	—	2·10
Wishaw and Coltness,	1·60	2·03
Brighton,	0·25	2·00
Slamannan,	—	1·88
East Lancashire,	—	1·85
London and South Western,	—	1·77
Lancaster and Carlisle,	—	1·75
Whitehaven,	—	1·60
Edinburgh and Glasgow,	—	1·50
Caledonian,	—	1·43
St. Helen's,	0·70	1·40
Llanelly,	—	1·35
Ipswich and Bury,	—	1·34
Shrewsbury and Chester,	—	1·33
Bristol and Birmingham,	—	1·32
Eastern Union,	—	1·26
York and Newcastle,	—	1·25
Newcastle and Carlisle,	—	1·25
Glasgow and Greenock,	0·81	1·25
Hartlepool,	0·75	1·25
Taff Vale,	0·66	1·16
Stockton and Hartlepool,	—	1·06
Lancashire and Yorkshire,	—	1·01
North British,	—	1·
Preston and Wyre,	—	1·
Eastern Counties (Cambridge)	—	1·
Clarence,	—	0·89
York and North Midland,	—	0·75

On the whole, the rates for the carriage of coal are lower than they were in 1845; only a small quantity of coal is carried on the higher priced lines.

The following shows the quantity of coal carried in the year ending June 30, 1847, at each rate of charge:—

3*d.* and upwards,	302,126 tons.
2*d.* and upwards,	198,252 „
1*d.* and upwards,	3,930,795 „
Under 1*d.*	629,416 „

Of the 3,930,795 tons carried at prices between 1*d.* and 2*d.*, 3,812,725 tons were carried at rates less than 1½*d.* per ton per mile. It will be seen that nearly all the coal carried by railway is carried for less than 1½*d.* per ton per mile.

The maximum charge for carrying coal is now 5·82*d.* per ton per mile, being on the West Cornwall or Hayle railway. In 1845 the maximum charge was 6*d.* per ton per mile, being on the Canterbury and Whitstable railway.

Many of the companies make no return of their charges for carrying coal, which is the more to be regretted, as the amalgamations have caused great alterations in the carrying rates.

The chief coal lines are the following:—

	1844-5. Tons.	1845-6. Tons.	1846-7. Tons.
York and Newcastle,	1,616,555	1,962,334	1,654,029
Stockton and Darlington,	900,000	904,358	911,645
Hartlepool,	796,486	893,701	789,673
Stockton and Hartlepool,	300,000	521,508	544,498
Midland,	492,420	395,325	481,344
London and North Western,	——	481,568	440,000
Caledonian,	——	227,183	335,319
Taff Vale,	125,066	284,066	314,621
St. Helen's,	229,775	245,573	247,734
Glasgow and Ayr,	120,000	180,130	242,443
Newcastle and Carlisle,	205,500	190,068	236,649
North Union,	321,923	395,021	233,137

The Ballochney, Monkland, and other railways in the neighbourhood, have a vast coal traffic; but from the state of the returns, no calculations can be made as to its extent.

The following shows the lines on which the largest receipts for coal have been obtained in each of the years ending June 30, 1845, 1846, and 1847:—

	1845. £.	1846, £.	1847. £.
York and Newcastle,	115,000*	123,050	112,858
Stockton and Darlington,	80,000*	65,736	71,842
Midland,	45,000*	71,851	64,250
London and North Western,	——	25,910	36,657
Stockton and Hartlepool,	——	31,043	33,558
Hartlepool,	32,627	35,958	31,477
Taff Vale,	19,939	24,447	28,620
Newcastle and Carlisle,	19,476	18,259	23,944
North Union,	——	20,000	20,000
Caledonian,	7,600	2,917	17,535

* Estimated amount.

It cannot be said that the coal traffic greatly advanced in the year ending June 30, 1847. Though a great advance was made in the previous year, there seem to be in 1847 symptoms of the effects of the commercial crisis. A decline took place in the receipts of the York and Newcastle, and Midland, though an increase took place on the Stockton and Darlington, and Newcastle and Carlisle.

The coals conveyed by railway from the several fields are as follows:—

	1845. Tons.	1846. Tons.	1847. Tons.
Northumberland and Durham,	3,850,000	4,500,000	4,250,000
Cumberland,	200,000	135,000	190,000
Leicester, Nottingham & Derby,	300,000	300,000	370,000
Lancashire,	900,000	1,270,000	1,000,000
Yorkshire,	——	240,000	190,000
Bristol,	50,000	90,000	100,000
South Wales,	220,000	380,000	400,000
Scotland,	760,000	1,650,000	2,100,000

It is impossible to separate the returns accurately, but it may be assumed that the quantity of coal carried by railway has increased in the Scotch and Cumberland fields. The produce of the Staffordshire fields cannot be separated.

The following shows the quantity of coals carried inland from the sea in each year:—

	1845. Tons.	1846. Tons.	1847. Tons.
Eastern Counties: Cambridge,	——	15,000	15,000
„ Colchester,	20,000	26,976	20,000
„ Eastern Union	——	——	16,000
„ Ipswich and Bury,	——	——	19,000
„ Norfolk,	——	6,000	15,000

South Eastern, ..	35,519	45,350	71,723
Brighton, ..	36,000	40,000	40,000
South Western, ..	4,000	18,830	31,659
London and North Western,	10,000	10,000	10,000
Cornwall, ..	28,000	30,065	33,545

The quantity of coal carried inland by means of railways has increased.

The saving in coal supplied to the city of York, in consequence of Mr. Hudson's railway measures, is not less than £30,000 yearly.

In the south-east of England, which is supplied mostly from the Northumberland and Durham coal-fields, the quantity carried upland by railway may be reckoned as follows:—

1845, 120,000 tons.
1846, 162,000 „
1847, 240,000 „

How trivial this is, may be seen from the quantity of coals imported into London, which is thus shown.

1825, 1,921,091 tons.
1835, 2,299,816 „
1845, 3,461,199 „

It will be seen that in the supply of the inland districts the railways have proved very effective; but they have as yet done very little for those districts which are supplied from the sea. This must be attributed to the inability of the railway companies to give their attention at present to this branch of traffic; but when they do, a complete revolution must be effected in the supply of coals. The only lines from which we have detailed returns are the South Eastern and South Western, which show a great increase in the quantity of coal carried. There is every reason to believe that a great traffic is growing up in the Eastern Counties district—not less than 85,000 tons: but the returns do not show this fully.

No. IV.—COKE TRAFFIC.

The traffic in coke must be considerable in some districts, but there is a want of detailed returns. It is a lucrative branch of revenue to the Midland Railway Company.

The following shows the traffic in coke in each year on the under-mentioned lines:—

	1844. Tons.	1845. Tons.	1846. Tons.	1847. Tons.
Midland, ..	26,826	29,767	57,015	78,246
Manchester and Bolton,	——	——	2,200	——
Newcastle and Carlisle,	3,504	8,333	3,312	8,221
York and North Midland,	——	16,013	50,785	93,899

The following shows the receipts for coke:—

	1846.	1847.
Midland, ..	£20,210	£24,198
Manchester and Bolton,	16	——
Newcastle and Carlisle,	391	457
York and North Midland,	4,772	7,792

No. V.—IRONSTONE AND IRON TRAFFIC.

These form great branches of mineral traffic, but the extent of them is very imperfectly expressed in the returns.

The traffic in ironstone on the following railways in the years ending June 30, 1845, 1846, and 1847, is shown below in tons.

	1845. Tons.	1846. Tons.	1847. Tons.
Ballochney, ..	239,010	190,352	229,362
Monkland, ..	——	200,000*	200,000*
Newcastle and Carlisle,	——	7,000†	7,000*
Taff Vale, ..	58,850	49,231	54,614
Ditto, Aberdare,	——	——	4,546
Furness, ..	——	——	106,301
Whitehaven, ..	——	406	140‡
Wishaw and Coltness,	32,240	42,231	27,000§

* Estimated amount. † Half-year, 3,381 tons. ‡ Half-year, 72. § Half-year, 13,976.

The returns for the Ballochney railway include iron likewise.

The amount received in each of those years was as follows:—

	1845.	1846.	1847.
Ballochney, ..	£6,931	£5,353	£8,901
Furness, ..	——	——	7,221
Newcastle and Carlisle,	——	2,700*	——
Taff Vale, ..	6,786	5,907	6,457
Ditto, Aberdare,	——	——	170
Whitehaven, ..	——	11	——
Wishaw and Coltness,	191	526	500*

* Estimated amount.

The rates charged for the conveyance of ironstone and iron-ore are as follows:—

	Per ton per mile.
Ballochney, ..	2·25*d.*
Newcastle and Carlisle,	1·37
Taff Vale, ..	1·16
Whitehaven, ..	1·25
Wishaw and Coltness,	2·23

The traffic in iron for the three years is as follows:—

	1845. Tons.	1846 Tons.	1847. Tons.
Ardrossan, ..	7,881	14,065	40,000†
Glasgow and Ayr,	25,000	39,679	56,825
Lancashire and Yorkshire,	——	9,000*	15,001
Maryport and Carlisle,	——	2,937	1,652
Shrewsbury and Chester,	——	——	9,488
Slamannan ..	——	——	881
Taff Vale, ..	88,493	61,996	67,039
Whitehaven, ..	——	1,233	1,200‡
Wishaw and Coltness,	73,429	77,826	80,000§

* Half-year, 4,470. † Half-year, 19,738. ‡ Half-year, 636. § Half-year, 44,030.

The amounts received are as follows:—

	1845.	1846.	1847.
Ardrossan, ..	£ 305	£ 556	£ 781*
Glasgow and Ayr,	2,400	——	——
Lancashire and Yorkshire,	——	1,011*	3,705
Maryport and Carlisle,	——	557	297
Shrewsbury and Chester,	——	——	851*
Slamannan, ..	——	——	78
Taff Vale, ..	4,901	6,974	7,770
Whitehaven, ..	——	52	37*
Wishaw and Coltness,	2,096	3,371	1,441*

* Imperfect returns.

The iron carried is mostly pig-iron.

The rates for the carriage of iron are as follows:—

	Bar-Iron, Per mile per ton.	Pig-Iron, Per mile per ton.
Ballochney, ..	3*d.*	1·66*d.*
Lancashire and Yorkshire,	2 25	——
Maryport and Carlisle,	——	1·80
Taff Vale, ..	——	1·00
Wishaw and Coltness,	——	1·84
Whitehaven, ..	——	1·88

A considerable quantity of dross and slag are carried, but there are only returns on the Wishaw and Coltness railway:—

Dross.

	1846. Tons.	1847. Tons.	1846. £	1847. £
Wishaw and Coltness,	110,084	90,000*	1,968	2,000

Slag and Char.

Wishaw and Coltness,	882	1,000†	9	30

* Half-year, 45,261 tons, 1,002*l.* † Half-year, 544 tons, 17*l.*

The rates for carrying dross are 1·12*d.* per mile per ton, and for carrying slag 1·35*d.* per mile per ton.

The traffic of the Glasgow iron district in coals, limestone, ironstone, iron dross, and slag carried by railway was in 1846 and 1847 as follows:—

	1846.	1847.
Tons,	2,500,000	2,900,000

The traffic of the Welsh iron district in coals, limestone, ironstone, and iron, stands as follows:—

	1844.	1845.	1846.	1847.
Tons,	242,000	323,000	396,000	441,000

The traffic of the Furness iron district consists solely in the shipment of ironstone to South Wales, to the extent of 106,301 tons in 1847.

No. VI.—COPPER AND TIN TRAFFIC.

The traffic in copper and tin ores is confined to the Cornish lines. There are no data now to show this. In 1845 I estimated it as follows:—

	Tons.	£	Rate.
Bodmin and Wadebridge,	3,000	200	3·00*d.*
Hayle, ..	20,000	4,000	4·20

No. VII.—LIMESTONE AND LIME.

The following shows the quantity of lime carried in the years ending June 30, 1846 and 1847:—

	1846. Tons.	1847. Tons.
Arbroath and Forfar,	1,000	——
Middlesborough and Redcar,	——	808
Maryport and Carlisle,	2,549	2,844
Leicester and Swannington,	2,727	——
Newcastle and Carlisle,	2,632*	——
Slamannan, ..	4,000	——
York and North Midland,	——	4,669

* Imperfect return.

The following shows the amounts received for the carriage of lime:—

	1846.	1847.
Leicester and Swannington,	£ 199	£ ——
Maryport and Carlisle,	108	122
Middlesborough and Redcar,	——	25
Newcastle and Carlisle,	89	——
York and North Midland,	——	830

The following shows the quantity of limestone and lime carried in the years ending June 30, 1844, 1845, 1846, and 1847:—

	1844. Tons.	1845. Tons.	1846. Tons.	1847. Tons.
Arbroath and Forfar,	——	——	1,000	——
Ballochney, ..	——	——	——	2,441†
Furness, ..	——	——	——	579
Great North of England,	2,500*	——	——	——
Llanelly and Llandilo,	338	291	——	——
Maryport and Carlisle,	821	2,261	2,549	2,844
Midland, ..	35,575*	56,290	73,776	56,677
Ditto (Leicester and Swannington)	2,848*	4,800	2,727	——
Middlesborough and Redcar,	——	——	——	818
Newcastle and Carlisle,	19,072*	40,260	——	——
Slamannan, ..	——	——	9,291	7,292
Wishaw and Coltness,	10,604	13,482	18,618	13,000‡
York and North Midland,	8,554	8,998	14,324	——
Ditto (Whitby and Pickering)	1,362	1,659	——	——

* Lime only. † Half-year, 1,257 tons. ‡ Half-year, 6,773 tons.

The traffic in limestone and lime in 1847, so far as details exist, may be calculated as follows:—

Arbroath and Forfar,	1,000 tons.
Ballochney, ..	5,000 „
Furness, ..	579 „
Llanelly and Llandilo,	2,500 „
Maryport and Carlisle,	2,844 „
Midland, ..	56,677 „
Middlesborough and Redcar,	808 „
Newcastle and Carlisle,	40,000 „
Slamannan, ..	7,292 „
Wishaw and Coltness,	18,000 „
York and North Midland,	14,000 „
York and Newcastle,	2,500 „
Total enumerated,	146,000 tons.

The total enumerated in 1845 was 132,544 tons.

Much of this is used for agricultural purposes; some for building; and some in the iron-works. The quantity carried for agricultural purposes may be reckoned thus:—

District.	Tons.
Scotland,	40,000
Northern,	50,000
Yorkshire,	25,000
Lancashire,	25,000
Midland,	50,000
Southern,	20,000
Total,	210,000

The amounts received for the carriage of limestone and lime in 1845, 1846, and 1847, were as follows:—

	1845.	1846.	1847.
Arbroath and Forfar,	£ 300	£ ——	£ ——
Ballochney, ..	——	——	66*
Furness, ..	——	——	2
Llanelly and Llandilo,	12	——	——
Maryport and Carlisle,	82	108	122
Midland, ..	6,020	9,622	5,369
Middlesborough and Redcar,	——	——	25
Newcastle and Carlisle,	3,774	——	——
Slamannan, ..	——	276	386
Wishaw and Coltness,	124	323	——
York and North Midland,	929	1,175	830*

* Lime only.

The total receipts for the carriage of limestone and lime in 1847 were not less than £11,000, but there has been a falling-off in the Midland traffic.

The rates for the carriage of a ton of limestone and lime per mile are as follows:—

	Lime.	Limestone.
Arbroath and Forfar,	2·5*d.*	——*d.*
Llanelly and Llandilo,	——	1·00
Maryport and Carlisle,	1·50	1·30
Lancashire and Yorkshire,	1·33	——
Newcastle and Carlisle,	1·25	1·25
Slamannan, ..	1·9	——
Wishaw and Coltness,	——	1·35
York and North Midland,	1·50	1·50

The greatest traffic in limestone and lime is carried on by the following companies:—

	Tons.	£
Midland, ..	56,677	5,369
Newcastle and Carlisle,	40,260	3,774
York and North Midland,	14,000	830
Wishaw and Coltness,	13,000	323

The limestone traffic on the Midland is on the old North Midland line.

No. VIII.—STONE TRAFFIC.

The number of tons of building and paving stone carried in the years ending June 30, 1845, 1846, and 1847, distinguished in the returns, is as follows:—

	1845. Tons.	1846. Tons.	1847. Tons.
Arbroath and Forfar, ..	20,000	14,239	11,711
Chester and Birkenhead, ..	——	——	478
Maryport and Carlisle, ..	2,381	3,975	3,637
Midland (Leicester and Swannington)	10,412	8,203	——
Lancashire and Yorkshire (Preston and Wyre)	852	——	——
Preston and Longridge, ..	——	——	37,000†
St. Helen's and Runcorn,	17,169	——	——
Stockton and Darlington,	20,936	55,907	89,540
Stockton and Hartlepool,	——	18,249	7,388
„ „ (Clarence)	——	26,977	13,857
Wishaw and Coltness, ..	6,492	4,466	11,000‡
York and North Midland,	——	100,000*	91,349
Ditto (Whitby and Pickering)	30,465	——	——
York and Newcastle (Great North of England)	4,000	——	——
„ (North Shields Branch)	——	9,484	7,000§
Total enumerated,	112,000	266,000	404,000

* Half-year, 51,030 tons. ‡ Half-year, 5,291 tons.
† Half-year, 18,071 tons. § Half-year, 3,465 tons.

The figures above given by no means represent the gross traffic in building and paving stones for each year. From an examination of the detailed figures, there seems to have been a falling-off in the the use of building and limestones in 1847, although the gross quantity carried on railways increased.

The total quantity of building and paving stones, limestones, and lime carried on railways in 1847 was as follows, according to the enumerated returns:—

Building stones,	404,000 tons.
Limestones and lime,	146,000 „
	550,000 tons.

The enumerated traffic gives the enormous quantity of 550,000 tons of stones carried, but the whole quantity carried must be nearer 1,000,000 tons.

The amounts received for the carriage of building and paving stones stand thus:—

	1845. £	1846. £	1847. £
Arbroath and Forfar, ..	1,100	945	740
Chester and Birkenhead, ..	——	——	70†
Maryport and Carlisle, ..	——	183	162
Midland (Leicester and Swannington)	269	211	——
Lancashire and Yorkshire (Preston and Wyre)	91	——	——
Preston and Longridge, ..	——	——	2 400‡
St. Helen's and Runcorn, ..	674	——	——
Stockton and Darlington, ..	——	2,023	3,030
Stockton and Hartlepool, ..	——	304	123
„ „ (Clarence)	——	780	436
Wishaw and Coltness ..	151	83	170§
York and North Midland, ..	——	7,000*	9,453
Ditto (Whitby and Pickering)	1,116	——	——
York and Newcastle (Great North of England)	800	——	——
„ (North Shields Branch)	——	473	700‖

* Half-year, 3,925*l.* † Half-year, 38*l.* ‡ Half-year, 1,208*l.*
§ Half-year, 86*l.* ‖ Half-year, 392*l.*

The total receipts enumerated in 1847 were about £18,000, and those for limestone and lime £11,000; making about £30,000 enumerated.

The rates for the conveyance of building and paving stones are as follows:—

	Per mile per ton.
Arbroath and Forfar,	2 50*d.*
Maryport and Carlisle,	2·04
Lancashire and Yorkshire,	1·33
London and South Western,	2·50
Preston and Longridge,	3 00
St. Helen's and Runcorn,	1·12
Wisbaw and Coltness,	2·50
York and North Midland,	1·00

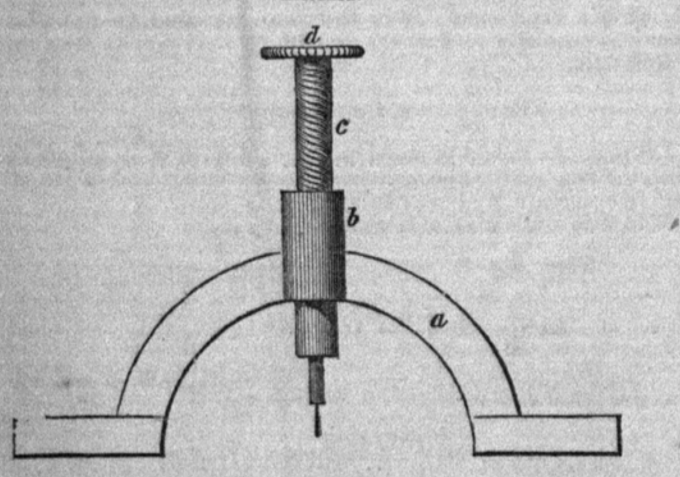
The greatest traffic in building, paving, and limestones, and in lime, is carried on by the following companies:—

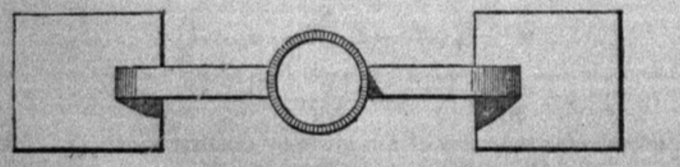
	Tons.	£
York and North Midland,	105,000	10,300
Midland ..	60,000	5,369
Newcastle and Carlisle,	40,000	3.800
Stockton and Darlington,	89,540	3.030
Preston and Longridge,	37,000	2,400
Stockton and Hartlepool,	21,245	560
Wishaw and Coltness,	24,000	500

PLAN-PRICKING INSTRUMENT.

Sir—Amongst other duties, I am engaged in making a most extensive and minute survey of a large city, showing every house and all the drainage throughout, the scale being very large—1 inch to 100 feet; and I am preparing duplicate fair copies of each sheet, which is being done by pricking through all the sheets at once, the original working drawing being placed uppermost. In doing this with the common pricker, I found that the draughtsman did not hold the pricker perpendicular; consequently, the lower sheets could not be accurate copies of the original. To obviate this, I have contrived an instrument, by which any attention on the part of the draughtsman in keeping it perpendicular is not required: all he has to do is to be careful that he pricks through the proper points of the plan correctly; the holes are then sure to be vertically under one another, let the sheets of paper be ever so numerous.

Elevation.





Plan.

The engraving is a plan and elevation of the instrument, the full size. The arch *a*, is of brass, with a cylindrical crown *b*, in which a piston *c*, works. At the lower end of the piston the needle-point is fixed; the arch is moved over the paper until the needle-point is precisely over the spot to be pricked through; the finger then presses on *d*, the top of the piston, which effects the puncture; and upon relieving it of the pressure, the spiral spring wound round the piston immediately raises it, and withdraws the needle-point from the paper;—in this way the work is done both correctly and rapidly.

To prevent the needle passing far through the paper and making a large hole, or sticking into the drawing-board, I have had a sheet of zinc to cover the board, and fastened down to it. This zinc sheet being a little less in size than the drawing-paper, admits of the latter being all (one upon the other) pinned down to the board round their edges, which overlap the edges of the zinc. I find the zinc to be very advantageous in use, as it causes the needle to make no other than extremely fine holes.

* * *

STREET PAVING.

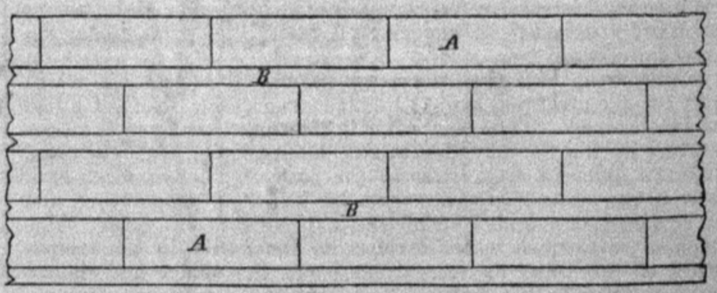
Sir—It is not necessary at this time to revive the controversy formerly raised between the respective advocates of wood and stone paving; experience or experiment—that great test of truth—will, ere this, have determined the opinions of most persons, as to the relative value or conditions of applicability of the two systems. It may not, however, have been considered how far combinations of the two might be made with advantage—adapted especially to certain cases.

The repairing of Holborn-hill, consequent on the recent removal of the houses on the north side of Holborn-bridge, brings to consideration the inconveniences—nay, dangers, occurring to the carriage traffic on declivities such as this, with any mode of paving hitherto adopted.

The placing of the granite stones obliquely to the line of surface, whereby the edges of the stones formed a series of sharp angles or steps (thus giving a rough surface and good foot-hold for horses drawing up, and obstructions to the too rapid sliding of wheels down the hill), appeared a vast improvement—and truly would be so, if the uneven surface thus obtained were not rapidly worn off by the continued traffic, and especially by the action of the drags on the wheels of carriages descending the hill. Thus the surface soon becomes smooth and slippery, whatever be the material used or form of laying, whenever the material is of one and the same kind, and consequently of equal wear throughout.

If, however, we employed materials, in conjunction, of different characters and rates of wear, we should then maintain inequalities of surface, affording continuous foot-hold for horses, and obviating existing defects.

Suppose that between each course of granite paving there were placed a thin course of wood, then the difference in the wear of the two would give the effect desired.



The annexed sketch shows the arrangement. A course of granite stones, A, to be laid in the usual way across the road, being about three inches in thickness; following this with a plank of wood, B, one inch thick, and of the depth of the granites; then following on with stones and planks in alternate courses.

Let us not stand arguing doubts and probabilities, but put the question to the true test—experiment. This can be done within a small space, and at little cost.

J. L.

Hampstead, August 8, 1848.

[A plan something similar to the above has been tried in Cheapside and in Piccadilly, but neither was of wood. They both produced a very disagreeable rattling and jolting in the carriage.]

EXPERIMENTS ON COALS.

Report on the Coals Suited to the Steam Navy. By Sir HENRY DE LA BECHE and Dr. LYON PLAYFAIR.

Experiments necessary to ascertain the true practical value of coal involve a very large series of observations, extended over a considerable period, and directed to special objects of inquiry. The qualities for which particular kinds of fuel are pre-eminent being so varied, it is impossible to deduce general results from a limited series of observations. Even in the economical application of coals, their evaporative value, or their power of forming steam, one variety of coal which may be admirably adapted from its quick action for raising steam in a short period, may be far exceeded by another variety, inferior in this respect, but capable of converting a much larger quantity of water into steam, and therefore more valuable in the production of force. A coal uniting these two qualities in a high degree might still be useless for naval purposes, on account of its mechanical structure. If the cohesion of its particles be small, the effect of transport or the attrition of one coal against another by the motion of a vessel might so far pulverise it as materially to reduce its value. Even supposing the three qualities united, rapidity and duration of action with considerable resistance to breakage, there are many other properties which should receive attention in the selection of a fuel without the combination of which it might be valueless for our steam navy.

There is an important difference existing between varieties of coals in the bulk or space occupied by a certain weight. For the purposes of stowage-room this cannot be ascertained by specific gravity alone, because the mechanical formation of the fragments of coal may enable one of less density to take up a smaller space than that occupied by another of a higher gravity. This is far from an imaginary difference, being sometimes as great as 60 per cent., and not unfrequently 40 per cent. The mere theoretical determination of the density of coals would, therefore, give results useless for practice. The space occupied between two varieties of coals, often equally good as regards their evaporative value, differs occasionally 20 per cent.—that is, where 80 tons of one coal could be stowed, 100 tons of another of equal evaporative value might be placed, by selecting it with attention to its mechanical structure.

These facts are mentioned merely to show that a hasty generalization should not be made, and to account for our drawing attention to these various points as a means of preventing the selection of a fuel from any one quality. We do not, in the present stage of this inquiry, consider it proper to offer any recommendation of our own as to particular kinds of fuel, leaving the experimental facts to decide for themselves.

After preliminary experiments had proved that no practical result could be attained by mere laboratory research, it was determined to test each variety of coal on a scale of sufficient magnitude to check the theoretical views by the practical results. As it was impossible for either of us to devote our whole time to this inquiry, our services being required by other official duties, we appointed assistants* to superintend its special parts, under our general direction.

It will be obvious that there are several circumstances which must receive attention before the true evaporative value of a fuel can be obtained. Thus, the water in the tanks has a varying temperature during the day, dependent on atmospheric changes, and is always different from that in the boiler. The temperature of water in the boiler also varies with the external temperature, and the circumstances under which the experiments are made. The shape of a Cornish boiler favours an inequality in the temperature of the water in its various parts, the colder and denser water sinking to the bottom, and having a tendency to remain there; so that the temperature of water at the surface is far from being the mean temperature of water in the boiler, the difference between the surface and bottom water being, on an average, 70°. Other circumstances naturally affect the evaporative powers of the coal, as for example the fact that all the water exposed to the action of the fire in the boiler is not converted into steam, and that wood is used to light the fire. Another circumstance of considerable importance, is the expansion or contraction of the boiler from an increase or diminution of the temperature. In the early stage of the experiments, those conducted by Messrs. Wilson and Kingsbury, it was thought unnecessary to make a correction for this variation in conditions; but on ascertaining experimentally that the difference was as much as 69·625 lb. of water in the contents of the boiler, between the temperature 150° and 212°, it became desirable to make an allowance for it, even when the difference between the initial and final temperature was not greater than 10°. Other circumstances of less importance, but influencing the results, have been neglected, because the application of such corrections would have only complicated the results, and would have had little practical value when the errors of observation in such approximative experiments remain so large. Among these may be mentioned the quantity of gases evolved during combustion, the elevation in temperature of the air entering the fire-place, the barometrical and hygrometric conditions of the atmosphere, the radiation from the boiler (very small in amount, owing to its brick covering), the hygrometric state of the fuel, or the heat necessary for obtaining mechanical draught in the chimney. In most of these cases the necessary observations have been made, to enable the corrections to be applied, should it afterwards appear desirable.

In making the calculation for the evaporative value of a fuel, the quantity consumed was divided into two portions, the first being that necessary to raise the whole mass of water exposed to the fire from the *mean temperature* to 212°, the second portion being that required to evaporate the water taken from the tanks from a temperature of 212°. To enable this to be done, the mean temperature of the whole mass of the water is ascertained—that is, the temperature of the water in the boiler at its initial temperature after being mixed with the tank-water at its average temperature. The average of the latter was the mean of several observations taken during the day, and is designated by t'.

Let w be the weight of water from the tanks at temperature t';

W the weight of water in the boiler at temperature t'', this being obtained from surface temperature corrected by experiment;

t, temperature after mixture.

$$\text{Then } t = \frac{W t'' + w t'}{W + w}.$$

The correction for the wood was made from data procured by Messrs. Wilson and Kingsbury, but it can only be employed for the particular wood used, as in subsequent experiments the evaporative value was found very different from another quality obtained. The co-efficient of the evaporative power of the wood may be deduced from experiment, in which a certain weight of water was raised from a known temperature to the boiling point, and then a certain portion of it evaporated. The following formulæ have been used by Mr. Kingsbury for the calculation:—

N is the total weight of wood used in raising $(W + w)$ (the weight of water in the boiler, and of that let down from the tanks during the experiment) from the mean temperature t to 212°; then it is necessary to find the weight N′ necessary to evaporate w from 212°.

$$\text{Then } \frac{w}{N'} = e, \text{ the evaporating power.}$$

Let m be the weight of wood required to raise $W + w$ from t to 212°, the number 1000 being assumed as the latent heat of steam.

N to evaporate $W + w$ from 212°

N′ to evaporate $W + w$ from 212°

$$\text{Then } m + N' = N. \qquad \text{Now } \frac{l}{212 - t} = \frac{n}{m}.$$

$$\text{But } \frac{n}{N'} = \frac{W + w}{w}; \quad \therefore N' = n \frac{w}{W + w};$$

$$l (N - N') = (212 - t)\, n = (212 - t)\, N' \left(\frac{W + w}{w}\right);$$

$$N l = N' \left\{\frac{W + w}{w} (212 - t) + l\right\}$$

$$= \frac{N'}{w} \left\{(212 - t)(W + w) + l\right\};$$

$$\therefore \frac{w}{N'} = \frac{(212 - t)(W + w) + l w}{N l} = e$$

or, introducing the value of t as given by the first formula,

$$\frac{(l + 212 - t')\, w + (212 - t'')\, W}{N l} = e.$$

If q be the quantity of wood used in lighting the fire, $e q$ will be the weight of water evaporated from 212° by the wood, and must be deducted from the weight of water evaporated in calculating the work done by the coal.

The co-efficient of the evaporating power of the coals, or the number of lbs. of water which one lb. of coal will evaporate from 212°, may be calculated as follows :—

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* The assistants employed were Mr. Wilson, Principal of the Royal Agricultural College, Mr. Kingsbury, Mr. J. Arthur Phillips, and Mr. Hutchinson. Mr. Wrightson, a pupil of Liebig, was entrusted with the analyses of the coal, Mr. Galloway analysed the gases, and Mr. Howe also assisted in the analyses.

Let P be the total quantity of coal consumed, then the work done by P will be to raise $W + w$ of water from t to 212°, and to evaporate $w - eq$ from 212°.

Let m be the weight of coal required to raise $W + w$ to 212°, from t
p ,, ,, evaporate $w - eq$ from 212°
n ,, ,, ,, $W + w$ from 212°

$$\text{Then } \frac{w - eq}{p} = \text{E, the evaporating power.}$$

$$\text{Now } P = m + p; \quad \frac{212 - t}{l} = \frac{m}{n}.$$

$$\text{But } \frac{p}{n} = \frac{w - eq}{W + w}; \quad \therefore l\left(\frac{w - eq}{W + w}\right) = p\,\frac{212 - t}{P - p}.$$

$$\frac{(W + w)(212 - t) + (w - eq)\,l}{P\,l} = \frac{w - eq}{p} = E.$$

Introducing the values from which the mean temperature t was obtained (first formula), we have eventually—

$$\frac{(l + 212 - t')\,w + (212 - t'')\,W - leq}{P\,l} = E$$

in which W is the weight of water in the boiler ;
w the weight of water drawn from the tanks;
t' the mean temperature of water in the tanks ;
t'' the corrected initial temperature of water in the boiler.

In the preceding formulæ, the latent heat of steam has been taken at 1000, the number generally used in this country; but after all the calculations had been made on this subject from the experiments by Messrs. Wilson and Kingsbury, and the results sent in to the Admiralty, Regnault's excellent memoir on the "Latent Heat of Steam" was published. It became necessary, therefore, to use these new results in the future experiments. These, so far as they apply to the present inquiry, are reduced in the following table.

TABLE I.—*Showing the Specific and Latent Heat of Water and Steam.*

Air Thermometer Centigrade.	Mercurial Centigrade.	Number of Unities of Heat abandoned by one kilo. of water in descending from T to 0°.	Air Thermometer Fahrenheit.	Mercurial Fahrenheit.	Number of Unities of Heat contained in one pound of water at T°.	Mean Specific Heat of Water between 0° and T cent. or between 32° and T Fahr.	Specific Heat of Water from T to T + dT.	Latent Heat of Steam saturated to the temperature T.	
								Centigrade.	Fahrenheit.
°	°		°	°					
0	..	0·000	32	..	32·000	..	1·0000	606·5	1091·7
10	..	10·002	50	..	50·003	1·0002	1·0005	599·5	1079·1
20	..	20·010	68	..	68·018	1·0005	1·0012	592·6	1066·7
30	..	30·026	86	..	86·046	1·0009	1·0020	585·7	1054·2
40	..	40·051	104	..	104·091	1·0013	1·0030	578·7	1041·6
50	50·2	50·087	122	122·36	122·156	1·0017	1·0042	571·6	1028·9
60	..	60·137	140	..	140·246	1·0023	1·0056	564·7	1016·4
70	..	70·210	158	..	158·381	1·0030	1·0072	557·6	1003·7
80	..	80·282	176	..	176·507	1·0035	1·0089	550·6	991·1
90	..	90·381	194	..	194·685	1·0042	1·0109	543·5	978·3
100	100·0	100·500	212	212·0	212·900	1·0050	1·0130	536·5	965·7
110	..	110·641	230	..	231·153	1·0058	1·0153	529·4	952·9
120	..	120·806	248	..	249·450	1·0067	1·0177	522·3	940·1
130	..	130·997	266	..	267·794	1·0076	1·0204	515·1	927·2
140	..	141·215	284	..	286·187	1·0087	1·0232	508·0	914·4
150	150·0	151·462	302	302·0	304·632	1·0097	1·0262	500·7	901·2
160	..	161·741	320	..	323·133	1·0109	1·0294	493·6	888·5
170	..	172·052	338	..	341·693	1·0121	1·0328	486·2	875·1
180	..	182·398	356	..	360·316	1·0133	1·0364	479·0	862·2
190	..	192·779	374	..	379·002	1·0146	1·0401	471·6	848·9
200	200·0	203·200	392	392·0	397·760	1·0160	1·0440	464·3	835·7
210	..	213·660	410	..	416·588	1·0174	1·0481	456·8	822·2
220	..	224·162	428	..	435·480	1·0189	1·0524	449·4	808·9
230	..	234·708	446	..	454·474	1·0204	1·0568	441·9	795·4

It also became desirable to introduce new corrections, which the progress of the inquiry showed to be needful. Thus, Mr. Phillips's careful experiments determined the alteration in the capacity of the boiler at different temperatures, and correction was in future made for this difference. The alteration in the capacity of the measuring tanks was also estimated, whenever the temperature differed 2° from that at which they were gauged. Another cause of error, for which allowance should be made, is any difference which may exist between the initial and final temperature at the beginning and close of the experiment. This difference being known by observation, the correction may be applied from the table of expansion of the water in the boiler, given. Introducing these new corrections into the experiments for ascertaining the co-efficient of the heating power of the wood, the following are the formulæ used by Mr. Phillips :—

$$\frac{(W + w - w')(l + t) + w\,t' + (w' - w)\,t''}{P\,l} = E.$$

In which W is the water let down from the tanks during the experiment.

w = The weight of water found in the boilers at commencement of experiment.

w' = The weight of water in boiler at close of experiment.

l = Co-efficient of the latent heat of steam.

t = Quantity of heat necessary to raise the water in tanks from its mean temperature to that at which it is evaporated.

t' = Quantity of heat necessary to raise the water in the boiler from the initial to the final temperature.

t'' = Quantity of heat necessary to raise water at the temperature of tanks to the final temperature of water in the boiler.

P = Weight of combustibles consumed during experiment.

E = The co-efficient of the heating powers of wood.

But when the initial is lower than the final temperature, the formula becomes—

$$\frac{(W + w - w')\,l + Wt + wt' + (w' - w)\,t'''}{P\,l} = E.$$

All the terms retaining their original value except the last, in which t'' is replaced by t''' (or the heat necessary to raise the final temperature to that at which the water was expanded), and must be regarded as having a negative value, while t' becomes positive. If now q is the weight of wood used in lighting the fire, the formula for estimating the evaporative power of the coal will be

$$\frac{(W - Eq + w - w')\,l + (W + w - w')\,t + wt' + (w' - w)\,t''}{P\,l} = E'$$

$$\text{And } \frac{(W - Eq + w - w')\,t + Wt + wt' + (w' - w)\,t'''}{P\,l} = E'.$$

As the experiments are strictly comparative, and under like conditions, the want of the other corrections, to which we have alluded above, will not be felt in examining the results; while their execution would have introduced a refinement into the experiments which never could be obtained in practice, and which, in fact, would be useless and unwarrantable while, as previously remarked, the errors of observation in all such approximative experiments remain so large.

The only omitted correction which in appearance might be supposed necessary for practical purposes, is that for the hygroscopic condition of the fuel. Had wood been employed, this must have been done; but the hygroscopic nature of coal is very much less than that of wood. The latter contains ⅕ its own weight of hygroscopic water; and the heat necessary for the evaporation of this quantity might be shown by a simple calculation to be nearly equal to 22 per cent. of the total heat obtained by the combustion of the wood. The hygroscopic water in coal is however very small, as will be seen by the following determinations of some of the Welsh specimens experimented upon :—

	Hygroscopic water.
Graigola Coal	1·06 per cent.
Anthracite	2·44 ,,
Oldcastle	0·74 ,,
Ward's Fiery Vein	1·27 ,,
Mynydd Newydd	0·67 ,,
Pentrepoth	0·78 ,,
Pentrefelin	0·70

Had we introduced corrections for these small quantities, practice would have been misled; because the coals will rarely reach a vessel in the dry state that they did in the present case, when they were packed in hogsheads and kept under cover.—It was found unnecessary to correct for any inflammable gases flying up the chimney, because repeated analyses of the chimney gases proved them not to contain any combustible constituent; the only products ever found being carbonic acid, sulphurous acid, oxygen, and nitrogen. The quantity of free oxygen in the chimney varied from ¼ to ½ of the oxygen which combined with the fuel; in other words, nearly twice the quantity of air passes through the fire than that which is strictly necessary by theory.

With regard to the selection of the coals for trial, we have to refer to Mr. Wilson's letter. This letter gives the information obtained in a tour made by Professor Wilson for the purpose of ascertaining the best coals fitted for trial in the South Wales coal district, and the ports from which they can conveniently be shipped. This district was selected because the varying character of the coals, from the bituminous to the anthracitic, offered those which were most likely to combine the qualities desired for naval purposes. It was intended, as being most convenient for the inquiry, to have adhered strictly to districts. In the experiments this has hitherto been done, except in special cases at the request of the Admiralty.

The following table (Table II.,) contains an abstract of the results, so far as regards the evaporative value of the fuel. This table relates only to the economical value of the coals examined, and to the steam generated by a unit of the respective coals, without however implying a unit of time. The details with reference to time, which forms a most important element in the value of the respective fuels, will be given hereafter.

Table II.—*Showing the Economic Values of the Coals.*

Names of Coals employed in the Experiments.	Economic evaporating power, or number of pounds of Water evaporated from 212° by 1 lb. of Coal.	Weight of 1 cubic foot of the Coal as used for fuel.	Weight of 1 cubic foot as calculated from the density.	Ratio of B. to C., or of the economical to the theoretical weight.	Difference per cent. between theoretical and economical weights.	Space occupied by 1 ton in cubic feet (economic weight).	Results of experiments on cohesive power of Coals per centage of large Coals.	Evaporating power of the Coal after deducting for the combustible matter in the residue.	Weight of Water evaporated from 212° by 1 cubic foot of Coal.	Rate of evaporation, or number of lbs. of Water evaporated per Hour.
WELSH COALS.	A.	B. lb.	C. lb.	D.	E.	F.	G.	H.	I.	Mean.
Graigola	9·35	60·166	81·107	·742	34·8	37·23	49·3	9·66	581·20	441·48
Anthracite, Jones	9·46	58·25	85·786	·679	47·26	38·45	68·5	9·7	565·02	409·37
Oldcastle Vein..	8·94	50·916	80·42	·633	57·946	43·99	57·7	..	455·18	464·30
Ward's fiery vein	9·40	57·433	83·85	·685	46·	39·	46·5	10·6	608·78	529·90
Binea	9·94	57·08	81·357	·702	42·53	39·24	51·2	10·3	587·92	486·95
Llangennech ..	8·86	56·93	81·85	·695	43·76	39·34	58·5	9·2	523·75	373·22
Pentrepoth ..	8·72	57·72	81·73	·705	40·17	38·80	46·5	8·98	518·32	381·59
Pentrefelin ..	6·36	66·166	84·726	·781	28·051	33·85	52·7	7·4	489·62	247·24
Duffryn	10·14	53·22	82·72	·643	55·43	42·09	56·2	11·80	540·12	409·32
Mynydd Newydd	9·52	56·33	81·73	·689	45·09	39·76	52·7	10·59	536·26	470·69
Three-quarter Rock Vein ..	8·84	56·388	83·60	·674	48·26	39·72	52·7	..	498·46	486·86
Cwm Frood Rock	8·70	55·277	78·299	·706	41·648	40·52	72·5	9·35	480·90	379·80
Cwm Nanty-gros	8·42	56·0	79·858	·701	42·60	40·00	55·7	8·82	471·52	404·16
Resolven	9·53	58·66	82·354	·712	40·39	38·19	35·0	10·44	559·02	390·25
Pontypool.. ..	7·47	55·7	82·35	·676	47·845	40·216	57·5	8·04	416·07	250·40
Bedwas	9·79	50·5	82·6	·611	63·565	44·32	54·0	9·99	494·39	476·96
Ebbw Vale ..	10·21	53·3	78·81	·676	45·98	42·26	45·0	10·64	544·19	460·22
Porthmawr ..	7·53	53·3	86·722	·614	62·7	42·02	62·0	7·75	401·34	347·44
Coleshill	8·0	53·0	80·483	·658	51·85	42·26	62·	8·34	424·0	406·41
SCOTCH COALS.										
Dalkeith Jewel	7·08	49·8	79·672	·625	59·984	44·98	85·7	7·10	352·58	355·18
,, Coronation	7·71	51·66	78·611	·657	52·17	43·[illegible]6	88·2	7·86	398·29	370·08
Wallsend Elgin	8·46	54·6	78·611	·694	43·78	41·02	64·	8·67	460·82	435·77
Fordel Splint ..	7·56	55·0	78·611	·699	4[illegible]·92	40·72	63·	7·69	415·80	464·98
Grangemouth ..	7·40	54·25	80·48	·674	48·35	40·13	69·7	7·91	401·45	380·40
ENGLISH COALS.										
Broomhill ..	7·3	52·5	77·988	·673	48·55	42·67	65·7	7·66	383·25	397·78
Lydney ..	8·52	54·444	80·046	·68	47·02	41·14	55·0	8·98	463·86	487·19
IRISH.										
Slievardagh ..	9·85	62·8	99·57	·630	58·55	35·66	74·	10·49	618·58	473·18
PATENT FUEL.										
Wylam's	8·92	65·08	68·629	·948	5·45	34·41	..	9·74	480·51	418·89
Bell's	8·53	65·3	71·124	·918	8·91	34·30	..	8·65	557·0	549·11
Warlich's.. ..	10·36	69·05	72·284	·955	4·49	32·44	..	10·60	715·36	457·84

The economical results obtained by evaporation in the best-applied practice are ascertained to be only a small part of the theoretical result following from the actual quantity of heat capable of being generated. Still, as a comparative statement, it is necessary to contrast the economical heat given out by a coal with the theoretical quantity. The cause of the difference between the applied and theoretical quantities is, at least in a great degree, obvious, and does not by the apparent difference prove the fallacy of calculation. Before the comparison can be made, it is necessary to have a knowledge of the composition of the respective coals: of this we subjoin a table.—(*See* Table III.)

Table III.—*Showing the Mean Composition of average samples of the Coals.*

Locality or name of Coal.	Specific Gravity.	Carbon.	Hydrogen.	Nitrogen.	Sulphur.	Oxygen.	Ash.	Per centage of Coke left by each Coal.
WELSH COALS.								
Graigola	1·30	84·87	3·84	0·41	0·45	7·19	3·24	85·5
Anthracite	1·375	91·44	3·46	0·21	0·79	2·58	1·52	92·9
Oldcastle Fiery Vein	1·289	87·68	4·89	1·31	0·09	3·39	2·64	79·8
Ward's Fiery Vein	1·344	87·87	3·93	2·02	0·83	*	7·04	..
Binea	1·304	88·66	4·63	1·43	0·33	1·03	3·96	88·10
Llangennech	1·312	85·46	4·20	1·07	0·29	2·44	6·54	83·69
Pentrepoth	1·31	88·72	4·50	0·18	..	3·24	3·36	82·5
Pentrefelin	1·358	85·52	3·72	Trace.	0·12	4·55	6·09	85·0
Duffryn	1·326	88·26	4·66	1·45	1·77	0·60	3·26	84·3
Mynydd Newydd	1·31	84·71	5·76	1·56	1·21	3·52	3·24	74·8
Three-quarter Rock Vein	1·34	75·15	4·93	1·07	2·85	5·04	10·96	62·5
Cwm Frood Rock Vein	1·255	82·25	5·84	1·11	1·22	3·58	6·00	68·8
Cwm Nanty-gros	1·28	78·36	5·59	1·86	3·01	5·58	5·60	65·6
Resolven	1·32	79·33	4·75	1·38	5·07	*	9·41	83·9
Ponty Pool	1·32	80·70	5·66	1·35	2·39	4·38	5·52	64·8
Bedwas	1·32	80·61	6·01	1·44	3·50	1·50	6·94	71·7
Ebbw Vale	1·275	89·78	5·15	2·16	1·02	0·39	1·50	77·5
Porthmawr Rock Vein	1·39	74·70	4·79	1·28	0·91	3·60	14·72	63·1
Coleshill	1·29	73·84	5·14	1·47	2·34	8·29	8·92	56·0
SCOTCH COALS.								
Dalkeith Jewel Seam	1·277	74·55	5·14	0·10	0·33	15·51	4·37	49·8
Dalkeith Coronation Seam ..	1·316	76·94	5·20	Trace.	0·38	14·37	3·10	53·5
Wallsend Elgin	1·29	76·09	5·22	1·41	1·53	5·05	10·70	58·45
Fordel Splint	1·25	79·58	5·50	1·13	1·46	8·33	4·00	52·03
Grangemouth	1·20	79·85	5·28	1·35	1·42	8·58	3·52	56·6
ENGLISH COALS.								
Broomhill	1·25	81·70	6·17	1·84	2·85	4·37	3·07	59·2
Park End, Lydney	1·283	73·52	5·69	2·04	2·27	6·48	10·00	57·8
Slievardagh (Irish)	1·59	80·03	2·30	0·23	6·76	*	10·80	90·1
FOREIGN COALS.								
Formosa Island	1·24	78·26	5·70	0·64	0·49	10·95	3·96	..
Borneo (Labuan kind)	1·28	64·52	4·74	0·80	1·45	20·75	7·74	..
,, 3 feet seam	1·37	54·31	5·03	0·98	1·14	24·22	14·32	..
,, 11 feet seam	1·21	70·23	5·41	0·67	1·17	19·19	3·23	..
PATENT FUEL.								
Wylam's	1·10	79·91	5·69	1·68	1·25	6·63	4·84	65·8
Bell's	1·14	87·88	5·22	0·81	0·71	0·42	4·96	71·7
Warlich's	1·15	90·02	5·56	Trace.	1·62	*	2·91	85·1

* Included in the Ash.

Chemists differ as to the mode of calculating the theoretical heating values of coals, but, as an approximative rule, without insisting on its absolute accuracy, their calorific values are found to stand in relation to the quantity of oxygen required for their complete combustion. This may be estimated experimentally by heating the coal with an excess of litharge; or it may be determined by calculation from the known equivalents of the combustible ingredients of the coal. From the quantity of lead reduced by the coal, the oxygen employed in its combustion may be estimated, and the calorific values stand in direct relation to this quantity. The amount of oxygen necessary to consume the combustible constituents may more accurately be determined by elementary analysis; and thus calculated, the results are generally found to be about $\frac{1}{3}$ greater than those indicated by experiment with the litharge. The calculation from the elementary analysis depends upon the circumstance, that 6 parts, or one equivalent, of carbon requires 16 parts, or two equivalents, of oxygen for combustion, while 1 part of hydrogen requires 8 parts of oxygen; it is only necessary, therefore, to subtract from the hydrogen a quantity corresponding to the oxygen contained in the coal to enable the calculation to be made on these principles.

As the calorific values are only relative, it is useful to refer them to the heating power of pure carbon, one part of which requires 2·666 parts of oxygen for combustion, and is capable, according to Despretz, of heating 78·15 parts of water from its freezing to its boiling point. The calculation may be simplified by multiplying each part of lead obtained by 2·265, which gives at once the weight of water capable of being heated between these temperatures by a unit of the coal used in reducing the litharge. On these principles the following table is constructed.—(*See* Table IV.

With regard to the practical application of fuel, such a table could not supersede experiment, as the economical values of the coal depend also on adventitious circumstances connected with their physical as well as their chemical condition. This table, while on the whole it agrees with and confirms the practical results of experiments, still differs in a marked degree in one or two instances: this difference arising as much from the chemical as from the physical differences of the coals. Thus, if by destructive distillation, which occurs in furnaces before combustion, a large quantity of the constituents of the coal are rendered gaseous, so much heat is expended in this act that the heat developed by their after-combustion is frequently not greater than that abstracted during their formation, in which case a thermo-neutrality occurs. To ascertain the proportion of fixed and volatile products in the various coals, a very difficult and elaborate process was adopted; but the tediousness and chances of failure in this kind of analysis have induced us to include only a limited number of coals (those given in Table V.), especially as for steam purposes it was sufficient to determine the per centage of coke, as stated in Table II.

TABLE IV.—*Showing the Calorific Values of the Coals.*

Locality or Name of Coal employed in the Experiments.	Quantity of Lead reduced by one part of Coal.	Oxygen removed from Litharge by one part of Coal.	Quantity of Oxygen theoretically required by Carbon and Hydrogen.	Quantity of Oxygen required by Carbon alone.	Relative calorific values, Carbon taken as 100, calculated from A. and B.	Number of lbs. of Water which 1 lb. of Coal can raise from 22° Faht. to 212° Faht., calculated from A.
WELSH COALS.	A.	B.	C.	D.	E.	F.
Graigola	32·08	2·49	2·49	2·26	93·4	72·66
Anthracite (Jones and Co.)	33·48	2·60	2·69	2·43	97·5	75·73
Oldcastle Fiery Vein	31·42	2·44	2·71	2·34	91·5	71·16
Ward's Fiery Vein	31·46	2·44	2·65	2·34	91·5	71·25
Binea	31·64	2·46	2·72	2·36	92·2	71·66
Llangenneck	32·66	2·53	2·59	2·28	94·9	73·97
Pentrepoth	31·16	2·39	2·69	2·36	89·6	70·57
Pentrefelin	30·52	2·37	2·53	2·28	89·2	69·13
Powel's Duffryn	30·00	2·33	2·71	2·35	87·7	67·95
Mynydd Newydd	30·34	2·35	2·67	2·25	88·5	68·72
Three-quarter Rock Vein	26·62	2·06	2·34	2·00	77·2	60·29
Cwm Frood Rock Vein	28·30	2·19	2·62	2·19	82·5	64·10
Cwm Nanty-Gros	29·64	2·28	2·47	2·08	85·5	67·13
Resolven	32·16	2·50	2·49	2·11	93·7	72·84
Pontypool	27·46	2·13	2·55	2·15	80·2	62·19
Bedwas	28·20	2·19	2·60	2·15	82·1	63·87
Ebbw Vale	32·00	2·48	2·80	2·39	93·0	72·48
Porthmawr Rock Vein	24·78	1·92	2·33	1·99	72·0	56·12
Coleshill	26·14	2·03	2·28	1·96	76·1	59·21
SCOTCH COALS.						
Dalkeith Jewel Seam	26·42	2·05	2·24	1·98	76·8	59·84
,, Coronation Seam	24·56	1·96	2·32	2·05	73·5	55·63
Elgin Wallsend	29·06	2·25	2·38	2·02	84·7	65·82
Fordel Splint	29·00	2·25	2·47	2·12	84·7	65·68
Grangemouth	28·48	2·20	2·46	2·13	82·8	64·51
Broomhill (English)	25·32	1·96	2·63	2·18	73·5	57·35
Slievardagh (Irish)	30·10	2·33	2·31	2·13	87·7	70·44
PATENT FUEL.						
Wylam's	28·32	2·23	2·52	2·13	84·0	65·27
Bell's	28·52	2·21	2·75	2·34	83·2	64·59
Warlich's	31·50	2·44	2·84	2·40	91·5	71·35

TABLE V.—*Showing the Amount of Various Substances produced by the destructive Distillation of certain Welsh Coals.*

Name.	Coke.	Tar.	Water.	Ammonia.	Carbonic Acid.	Sulph. Hydrogen.	Olefiant Gas and Hydro-Carbon.	Other Gases inflammable.
Graigola	85·5	1·2	3·1	0·17	2·79	Traces.	0·23	7·01
Anthracite	92·9	None.	2·87	0·20	0·06	0·04	?	3·93
Oldcastle fiery vein	79·8	5·86	3·39	0·35	0·44	0·12	0·27	9·77
Ward's fiery vein	..	1·80	3·01	0·24	1·80	0·21	0·21	..
Binea	88·10	2·08	3·58	0·08	1·68	0·09	0·31	4·08
Llangenneck	83·69	1·22	4·07	0·08	3·21	0·02	0·43	7·28

It has been for some time asserted, that the evaporative value of a bituminous coal is expressed by the evaporative value of its coke, the heat of combustion of its volatile products proving in practice little more than that necessary to volatilise them. If this supposition were even near the truth, the most useful practical results might follow from it. By a larger and better applied system of gas manufacture, the volatile products of distillation might be made useful not only for the purposes of illumination, but also for domestic heat, and the residual coke might be used with an equal economy in our manufactures*; thus preventing the emission of that smoke which at present is so destructive to the comfort of our large cities. It is easy from analysis to examine whether the duty performed by the coal is to be attributed to its fixed ingredients or coke, by estimating the work which the latter is capable of performing. This may be done by subtracting the amount of ashes in the coal from its amount of coke (Table III.) and estimating the remainder as carbon. This carbon multiplied by its heating power, 13268, and divided by 965·7 or the latent heat of steam, indicates the number of pounds of water which the coke by itself could evaporate, without the aid of the combustible volatile ingredients of the coal. These results are placed in column B, of Table VI., in juxta-position with the actual work done by the coal, and it will be seen, that notwithstanding several striking exceptions, which might have been expected, they on the whole show that the work capable of being performed by the coke alone, is actually greater than that obtained by experiments with the original coal.

The whole system of manufacturing coke is at present very imperfect. Besides losing the volatile combustible substances, which under new adjustments might be made of much value, an immense quantity of ammonia is lost by being thrown into the atmosphere. Ammonia and its salts are daily becoming more valuable to agriculture, and it is their comparative high price alone which prevents their universal use to all kinds of cereal cultivation. By a construction of the most simple kind, the coke ovens now in use might be made to economise much of the nitrogen which invariably escapes in the form of ammonia. As an inducement to this economy, we have appended to Table VI two columns (H. and I.), showing the quantity of ammonia (NH_3), and its corresponding quantity of commercial sulphate (NH_4O, SO_3), which each 100 lb. of the respective coals may be made to produce. When it is remembered, that the price of sulphate of ammonia is about £13 per ton, or that 100 tons in coking is capable of producing, on an average, about 6 tons of this salt, its neglect is highly reprehensible.

By the preceding data, the actual value of the coals will be contrasted with that which is theoretically possible, supposing their combustion proceeded under circumstances which prevented any loss of heat. The actual duty obtained by a pound of coal from the boiler employed may be easily expressed by the number of pounds raised to the height of one foot. This result may readily be obtained by the simple formula—

$$W\eta \times 965{\cdot}7 \times 782 = x,$$

W representing water, of which η pounds are evaporated by a pound of coal. This formula is deduced from the fact that η pounds of water multiplied by 965·7,* or the co-efficient for the latent heat of steam at 212°, indicates the number of pounds of water which would be raised 1° Fahrenheit; and the number 782 arises from experiment on the mechanical force denoted by the elevation of a pound of water 1° Fahrenheit; that force being equal to 712 lb., raised to the height of one foot, according to the careful experiments of M. Joule, on the friction of oil, water, and mercury.

The theoretical value of the coals, with reference to the number of pounds of water which one pound of fuel will convert into steam, is obtained by the formula—

$$\left(\frac{C \times 13268}{965{\cdot}7}\right) + \left(\frac{H - h \times 62470}{965{\cdot}7}\right) = x;$$

in which C is the quantity of carbon, H the quantity of hydrogen in a unit of fuel, and *h* the quantity of hydrogen corresponding to the oxygen contained in the coal. These multiplied by their heating powers, according to the results of Dulong, and divided by the latent heat of steam, indicate the number of pounds of water that can be converted into the latter by a pound of coal. The numbers thus obtained can be changed into the expression of mechanical force, by the previous formulæ.

The result of these calculations are thrown into Table VI.

The best Cornish engines are stated to raise 1,000,000 lb. to the height of one foot, by every pound of coal consumed; so that only about $\frac{1}{9}$ of the *actual* force generated becomes available, or only $\frac{1}{11}$ or $\frac{1}{12}$ of the force theoretically possible, is applied in practice. The various experiments made on boilers, with regard to the evaporative power of coal, have not given very uniform results. Smeaton, in 1772, with one pound of Newcastle coal, evaporated 7·88 lb. of water from 212°; Watt, in 1788, came to the conclusion that 8·62 lb. of water might be evaporated by the same quantity of coal; and later (in 1840), Wicksteed found that 1 lb. of Merthyr coal could be made to evaporate 9·493 lb. of water from 80°, which is equal to 10·746 lb. from 212°. In some experiments made on the boiler of the Loam's engine, at the United mines, in Cornwall, each pound of coal was found, by a trial of six months, to evaporate 10·29 lb. of water from 212°, this being the reduction of the result given, viz., that 234,210 cubic feet of water at 120° were evaporated by 700 tons of coal. Statements have indeed been made that 14 lb. of water have been evaporated by 1 lb. of coal burned in Cornish boilers; but as this is the utmost quantity theoretically possible, it is difficult to conceive that it has been realised in practice, even in the best-constructed steam-engines.

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* In this case it would not be necessary to carry on the process of distillation so far as at present, as the residual coke would be more combustible and the gases purer.

* The co-efficient for the latent heat of steam at 212° is generally taken at 1000°, but the above number is from the recent experiments of Regnault on this subject, as given in Table I.

TABLE VI.—*Showing the Actual Duty, and that which is theoretically possible, of the Coals examined.*

Name or Locality of Coal employed in the Experiments.	Actual number of lbs. of Water converted into steam by 1 lb. of Coal.*	Number of lbs. of Water convertible into Steam by the Coke left by the Coal.†	Number of lbs. of Water convertible into Steam by the Carbon of the Coal.†	Number of lbs. of Water convertible into Steam by the Hydrogen of the Coal.†	Total number of lbs. of Water convertible into Steam by 1 lb. of Coal.†	Actual force generated, or the number of lbs. which 1 lb. of Coal could raise to the height of 1 foot.‡	Force capable of being generated, or number of lbs. which could be raised to the height of 1 foot by 1 lb. of Coal.†	Amount of Ammonia corresponding to the Nitrogen contained in Coal.	Amount of Sulphate of Ammonia corresponding to the Nitrogen contained in Coal.
	A.	B.	C.	D.	E.	F.	G.	H.	I.
Graigola	9·35	11·301	11·660	1·903	13·563	7,060,908	10,242,471	0·497	1·932
Anthracite ..	9·46	12·554	11·563	2·030	14·593	7,143,978	11,020,303	0·225	0·990
Oldcastle Fiery Vein	8·94	10·601	12·046	2·890	14·936	6,751,285	11,279,329	1·590	6·175
Ward's Fiery Vein	9·40	..	12·072	2·542	14·614	7,098,667	11,036,162	1·238	4·808
Binea	9·94	11·560	12·181	2·912	15·093	7,506,463	11,397,892	1·586	6·741
Llangenneck	8·86	10·599	11·741	2·519	14·260	6,690,871	10,768,829	1·299	5·044
Pentrepoth ..	8·72	10·873	12·189	2·649	14·838	6,585,146	11,205,322	0·218	0·848
Pentrefelin ..	6·36	10·841	11·749	2·038	13·787	4,802,928	10,411,630	Trace.	..
Powell's Duffryn	10·149	11·134	12·126	2·966	15·092	7,664,295	11,397,137	1·76	6·835
Mynydd Newydd	9·52	9·831	11·463	3·441	14·904	7,189,288	11,255,163	1·808	7·340
Three-quarter Rock	8·84	7·081	10·325	2·781	13·106	6,675,768	9,897,355	1·299	5·044
Cwm Frood Rock	8·70	8·628	11·300	3·488	14·788	6,570,043	11,167,563	1·347	5·232
Cwm Nanty-Gros	8·42	8·243	10·767	3·165	13·932	6 358,593	10,521,131	1·919	7·448
Resolven ..	9·53	10·234	10·899	3·072	13·971	7,106,840	10,550,583	1·675	6·505
Pontypool ..	7·47	8·144	11·088	3·207	14·295	5,641,175	10,795,260	1·639	6·364
Bedwas ..	9·79	8·897	11·075	3·766	14·841	7,393,186	11,207,587	1·748	6·788
Ebbw Vale ..	10·21	10·441	12·335	3·300	15·635	7.710,361	11,025,198	2·622	10·182
Porthmawr Rock	7·53	6·647	10·263	2·548	12·811	5,686,485	9,674,577	1·554	6·033
Coleshill ..	8·0	6·468	10·145	2·654	12·799	6,041,419	9,665,515	1·785	6·930
Dalkeith Jewel ..	7·08	6·239	10·242	2·071	12·313	5,346,655	9,298,499	1·214	0·471
Dalkeith Coronation	7.71	6·924	10·570	2·202	12·772	5,822,417	9,645,125	Trace.	..
Wallsend Elgin	8·46	6·560	10·454	2·968	13·422	6,388 800	10,135.991	1·712	6·647
Fordel Splint	7·56	6·560	10·933	2·884	13·817	5.709,141	10,434,286	1·372	5·327
Grangemouth	7·40	7·292	10·970	2·722	13·692	5,588,312	10,339,888	1·639	6·364
Broomhill ..	7·30	7·711	11·225	3·638	14·863	5,512,795	11,224,201	2·234	8·674
Park End, Lydney	8·52	6·567	10·101	3·156	13·257	6,434.111	10,011,386	1·477	9·617
Slievardagh (Irish)	9·85	10·895	10·995	1·487	12·482	7,438,497	9.426,124	0·279	1·084
Formosa Island	..	..	10·752	2·801	13·533	..	10,234,919	0·777	3·017
Borneo (Labuan)	..	..	8·864	1·388	10·252	..	7 742,078	0·977	3·771
„ 3 feet seam	..	..	7·461	1·295	8·756	..	6,612,333	1·132	4·620
„ 11 „	..	..	9·652	1·948	11·600	..	8.760,057	0·813	3·158
Wylam's Pt. Fuel	8·92	8·378	11·186	3·145	14·331	6.736,182	10.822,447	2·040	7·920
Warlich's ..	10·36	11·292	12·368	3·596	15·964	7,823 637	12,055,652	Trace.	..
Bell's ..	8·53	9·168	12·074	3·343	15·417	6,441,663	11,642,569	0·983	3·818

* Practical. † Theoretical. ‡ Calculated from heat obtained.

To ascertain how far our boiler was inferior to Cornish boilers, as principally from its size and less efficient coating it was likely to prove, we requested Mr. Phillips to make some experiments on one of the best engines in Cornwall. It was found by these experiments, that 11·42 lb. of water were evaporated by every pound of Welsh coal corresponding in composition to that of Mynydd Newydd; or, in other words, that improved Cornish boilers on a large scale may be assumed to have a superiority of nearly 20 per cent. over that used in these experiments. As the results stated in this Report are only relative, the comparison is not affected by this difference.

We have anxiously looked to the application of these experiments to the different varieties of patent fuel, but we have not been able to carry our observations in this direction to the extent we could have desired, from our inability to procure patent fuels in sufficient number, although our applications to the patentees have been numerous. Three varieties have been already examined, viz., those manufactured under the patents of Messrs. Wylam, Warlich, and Bell, and the results are given in the tables. The varieties of patent fuel are generally made up in the shape of bricks, and are therefore well adapted for stowage; so that, though the specific gravity of patent fuels is lower than that of ordinary coals, from their shape and mechanical structure there are very few coals which could be stowed in a smaller space per ton. While we look to the different varieties of patent fuel as of the highest importance, and, from their facility of stowage, as being peculiarly adapted for naval purposes, and perhaps even destined to supersede ordinary coal, at the same time, the greater part do not appear to be manufactured with a proper regard to the conditions required for war steamers. It is usual to mix bituminous or tarry matter with bituminous coal, and from this compound to make the fuel. An assimilation to the best steam coal would indicate, however, the very reverse process, and point to the mixture of a more anthracitic coal with the bituminous cement. As the greater part is at present made, it is almost impossible to prevent the emission of dense opaque smoke, a circumstance extremely inconvenient to ships of war, as betraying their position at a distance at times when it is desirable to conceal it. Besides this and other inconveniences, the very bituminous varieties are not well suited to hot climates, and are as liable to spontaneous combustion as certain kinds of coal. To avoid these inconveniences, some kinds of patent fuels have been subjected to a sort of coking, and thus, in a great measure, obtain the desired conditions. There is little doubt, however, that notwithstanding the large number of patents in operation for the manufacture of fuel, its value for the purposes of war steamers might be much enhanced by its preparation being specially directed to this object. It will be seen, by reference to Table II., that the three patent fuels examined rank among the highest results obtained. Should it be desirable to continue this inquiry, we conceive that it would be advantageous to pay special attention to this subject, by experimenting upon proper mixtures of different coals. Even anthracite may be introduced into such mixtures with advantage.

It is of much importance in an economical inquiry on coals, to obtain exact information as to the effects likely to be produced upon them by stowage and continued exposure to high temperature, not only as regards their deterioration, but also as to the emission of dangerous gases by their progressive changes.

The retention of coal in iron bunkers, if these are likely to be influenced by moisture, and especially when by any accident wetted with sea-water, will cause a speedy corrosion of the iron, with a rapidity proportionate to its more or less efficient protection from corroding influences. This corrosion seems due to the action of carbon or coal forming with the iron a voltaic couple, and thus promoting oxidation. The action is similar to that of the tubercular concretions which appear on the inside of iron water-pipes, when a piece of carbon, not chemically combined with the metal, and in contact with saline waters, produces a speedy corrosion. Where the "make" of iron shows it to be liable to be thus corroded, a mechanical protection is generally found sufficient. This is sometimes given by Roman cement, by a lining of wood, or by a drying oil driven into the pores of the iron under great pressure.

Recent researches on the gases evolved from coal, prove that carbonic acid and nitrogen are constantly mixed with the inflammable portion, showing that the coal must still be uniting with the oxygen of the atmosphere, and entering into further decay.

Decay is merely a combustion proceeding without flame, and is always attended with the production of heat. The gas evolved during the progress of decay, in free air, consists principally of carbonic acid, a gas very injurious to animal life. It is well known that this change in coal proceeds more rapidly at an elevated temperature, and therefore is liable to take place in hot climates. Dryness is unfavourable to the change, while moisture causes it to proceed with rapidity. When sulphur or iron pyrites (a compound of sulphur and iron) is present in considerable quantity in a coal still changing under the action of the atmosphere, a second powerful heating cause is introduced, and both acting together, may produce what is termed *spontaneous combustion*. The latter cause is in itself sufficient, if there be an unusual proportion of sulphur or iron pyrites present.

The best method of prevention, in all such cases, is to ensure perfect dryness in the coals when they are stowed away, and to select a variety of fuel not liable to the progressive decomposition to which allusion has been made. This is, however, a subject of so much importance to the steam navy, that it continues to receive our careful attention; and, beyond these general recommendations, it would be premature to offer any decided course for adoption, from the present limited series of observations.

Several varieties of coal were transmitted from Formosa and from Borneo, for analysis, the results of which are contained in the accompanying table. The quantity of each kind was so small, that no experiments could be made on their evaporative value. We extract from the preceding table the following results:—

Name.	Carbon.	Hydrogen.	Nitrogen.	Sulphur.	Oxygen.	Ashes.	Specific Gravity.
Formosa Island ...	78·26	5·70	0 64	0·49	10 95	3·96	1·24
Borneo, Labuan kind	64 52	4·74	0·80	1·45	20·75	7·74	1·28
„ 3 feet seam	54·31	5·03	0·98	1·14	24·22	14·32	1·37
„ 11 feet seam	70·33	5·41	0 67	1·17	19·19	3·25	1·21

THE HOUSES OF PARLIAMENT.

That so important an edifice and national work as the "Houses" should be a frequent subject of discussion, both in parliamentary debate and out-of-door criticism, is only natural, especially as opinion is divided, some speaking in terms of unqualified admiration, while others see much cause for dissatisfaction, and express it accordingly; and some of the rudest critics of all whom Mr. Barry and his building have had to encounter, are among the members of the "House" itself—to wit, Messrs. Urquhart, Osborne, and Hume, and Sir D. Norreys, whose unfavourable opinions gain formidableness because they go forth to every nook in the kingdom where a newspaper finds its way. Last month we spoke of an article on the "Houses of Parliament," which had just before appeared in the "*Westminster Review*,"—and which, by-the-by, is now known to proceed from the pen of Earl Lovelace; and in the interval another has appeared in the "*Mechanic's Magazine*," of similar tendency. Whether this latter will call forth any remonstrance against it from the "*Builder*," remains to be seen; for at the present time of our writing, opportunity for reply to it has not arrived. The "*Magazine*" takes up the "*Builder*" pretty sharply upon two points. The first of them is the attempt on the part of the last-mentioned publication to set aside Lord Lovelace's objections to the position of the Victoria Tower at a corner, and the most remote corner, of the building, by remarking that such position for it was dictated by the plan adopted by Mr. Barry; whereupon the "*Magazine*" is somewhat sarcastic, and to say the truth, Mr. Barry has no cause to feel particularly grateful towards a defender who exculpates him, by removing the fault from him and throwing it upon—his design: as if defects of arrangement were to be attributed to the plan itself, and not to the architect. If the internal arrangement which first presented itself to the architect occasioned what is an incongruity in the exterior, the very natural question is: Why did he not, instead of adopting it, deviate from it so as to bring in the Royal entrance porch in some less objectionable situation?—and objectionable it seems to be, for even his champion, as the "*Mechanic's Magazine*" calls the "*Builder*," does not pretend to say that it is not so, but merely that it is to be excused by being attributed to—the plan!

The second point upon which the "*Mechanic's Magazine*" is rather strong and severe with the "*Builder*," is the unguarded assertion that, although it is so now, it does not follow that the Victoria Tower will always be at the extreme corner of the edifice. The actual possibility of extending the buildings and carrying them on farther southward, by pulling down the houses on the east side of Abingdon-street as far as might be required, is not to be disputed. The probability is a very different matter,—more than can now be foreseen, and is, besides, what Mr. Barry himself neither does nor ever has contemplated; the Victoria Tower being the conclusion and *finis* of his plan, southwards. As the "*Mechanic's Magazine*" remarks, the south-west is now finished, at least in its lower part, so as to render it impossible to prolong the building, and thereby remove that Tower from its position as an extreme point in it. Done it could be, but only by undoing what is equally beautiful and costly—namely, the south side of the Royal porch, which would have to be blocked-up and built up against.

The Royal entrance is now fixed beyond the possibility of change for it. Yet, it is not even now too late to re-consider some other points in the design. For instance, although the position of that important entrance may be justified by necessities of internal arrangement, and although it is very properly made a striking feature in the design, the necessity for erecting over it a tower of very unusual magnitude, and thereby proclaiming afar off the "eccentricity," as the "*Mechanic's Magazine*" terms it, of that porch, is not at all apparent; more especially, as that lofty superstructure will be more for sight than for any real service. We incline, therefore, to the opinion of the "*Athenæum*," that it would be more adviseable now to abandon the idea of that ambitious Tower, and terminate the Porch a little above its present height. Either some curtailment, we fancy, of Mr. Barry's plans must take place, or the ultimate completion of them must not be looked for by the present generation.

Could the Tower in question have been introduced in the centre of the general plan, it would have given not only pyramidal grouping to the whole pile, but harmonious contrast in its lines to the composition. Marked verticality of lines in such an imposing feature, so placed, would have been opposed to horizontality of lines in the principal front. The lofty upright mass and the horizontally-extended façade would have balanced each other, and mutually set each other off. Other towers there now will be, rising up behind the river-front; but how far they will agree with it, and with the larger tower also, is doubtful. If we mistake not, they are intended to be tapering and spiry in outline—consequently of quite a different character from the compact solidity which marks the Victoria one; therefore somewhat at variance with the character of the front also. At least, there is reason for apprehending that the arrangement of the several towers will appear very irregular, if not confused, and occasion an unpleasant discord with the studied regularity and uniformity of the principal front—principal at least in extent, though both its situation and its aspect render it in some degree a merely secondary one. If there must be a tower that shall, by its superior bulk, greatly predominate over all the others, it surely ought to show itself in some central situation,—central as regards one of the fronts, if not central as regards the entire plan; otherwise, it will appear to have been left to chance to determine and "dictate" the respective situations of those features, instead of their being arranged with some regard to that symmetry which is observed in the main, as far as it was possible to do, and which at present stamps the whole of the east side of the edifice.

It will perhaps be said that the position of the Victoria Tower was known from the very first: it has been shown again and again in the various engravings and cuts innumerable, copied or made up from the view of the future building given by the architect himself; its grandeur has been admired, without exception being taken at its situation. Yet, though no objection has been made all along, now comes Earl Lovelace with a very strong protest against that *Tower*. Wherefore was his Lordship so tardy with his remonstrances? or how happens it that no one else could perceive, or perceiving, cared to object to what is now alleged to be a most serious defect in the general design? The position of the Tower could hardly have been overlooked by, or indeed have failed to strike, the most careless observer who glanced at any of the published views. Very true; but merely seeing with the eyes, is quite a different matter from seeing with artistic vision. It is not every one who can *see*, in the latter sense of the word, what is actually before their eyes. Many, again, don't care to see, even if they can do so. Others, though they may be somewhat dim-sighted, have very convenient spectacles of criticism through which they gaze, and perceive either only all beauty or all deformity—either transcendant excellence, or ridiculous monstrosity.

The many, who have no opinion of their own, are overawed by this kind of dictatorial, one-sided stuff which calls itself criticism, and is presented to them in the imposing form of type and printed paper. While those who are capable of judging for themselves either do not care to raise a dissentient voice amidst the general hubbub of applause, or condemnation, as the case may be; or else have not the opportunity of doing so. With respect to the "Houses," both the "*Westminster Review*" and the "*Mechanic's Magazine*" have ventured to dissent from the acclamations of praise bestowed upon them in other quarters. It must be allowed that both those articles dwell almost exclusively upon defects, or what their writers consider such: yet surely there is nothing particularly strange in that, the object of both being to open people's eyes to many circumstances that have all along been kept out of sight. If to point out only faults and objections be invidious, by the same rule, to pass them entirely over, and point out only merits and beauties—would that all of them were where they could be seen!—may be called sycophantic: so that between the two we may arrive at a tolerably correct and sober judgment.

BOROUGH OF LIVERPOOL.

Report to the Health Committee of the Borough of Liverpool, on the Sewerage and other works under the Sanitary Act, by the Borough Engineer, (James Newlands.)

The facts detailed in the Report of the engineer of the Liverpool corporation afford a strong commentary on the claims of the military engineers. We have heard a great deal about the irresponsibility of the Associated Surveyors, and the superior responsibility and guarantee of the military engineers; and Mr. Chadwick rests the defence of his job on this plea. We have always held the contrary view, from our experience of the two classes, and this Liverpool affair comes in confirmation. The fact is, the military engineers are virtually irresponsible—they cannot be made to perform their work properly or punctually; while the civil engineer, at Liverpool for instance, is responsible in his professional character and capacity, and liable to be dismissed by his employer if he do not give satisfaction. From the misconduct of the Ordnance functionaries, the corporation of Liverpool are put to the trouble, expense, and delay of a second survey—and, to make it worse, they have no remedy. Whether the metropolitan survey will turn out better we have our doubts: but it remains to be seen. At all events, the Treasury will have to make good any short-comings of the Ordnance surveyors.

If the Ordnance have been unable to do their work properly and in time at Liverpool, what security have we against delay in the general and metropolitan surveys? To the great disgrace of the government, engineers have long had to use the northern sheets of Mr. Cary's survey; and now this dilatory body, having proved itself incompetent in its past duties, is to have more thrust upon it. Why do not the Associated Surveyors get Mr. Wyld to move for a parliamentary committee of inquiry into the general management of the surveys carried on by the Ordnance?

This Report is a sufficient specimen to show that civil engineers are not incompetent for sanitary duties, and we hope it will not be forgotten, for doubtless Mr. Chadwick will next propose that military engineers should be chosen to lay down the lines of drainage, and carry out the details under the "Health of Towns" Act.

The Report may be divided into the following parts:—

Sewage and drainage, what are the necessary conditions to produce a perfect system.

Description of the borough of Liverpool in relation to the river Mersey and the docks—the effects of discharging the sewage into the docks and river.

Schemes for constructing a new sewage.

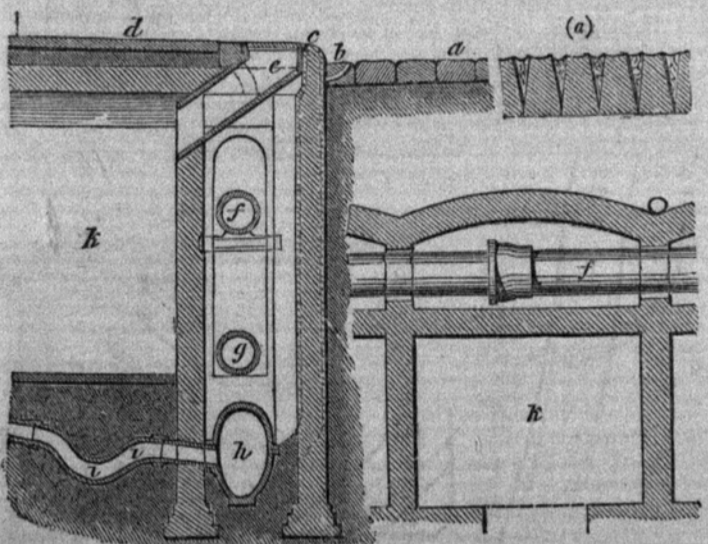
Lists of all the existing sewers, showing their size and length—of new sewers required.

Estimates for forming the new system of sewage.

On the application of the sewage water for fertilizing the soil.

On the form and size of sewers and drains. From this part of the report we make the following extracts:—

Sewers and Drains.—The proper size and forms of sewers and drains is a subject which of late has excited much controversy. In so far as the house drainage is concerned, the question lies within narrow limits; for if all the liquid refuse of a house passes through a soil-pipe of 2½ or 3 inches diameter, there is surely no need of the drain which receives it being made much larger. If more than one pipe enters a drain, the question is still a simple one; and although, by calculation, the corresponding increase of capacity for every additional pipe which enters the drain could be readily determined, practice will hardly admit of the refinement which would so nicely adjust the increments; and it is easy so to proportion the drain pipe in each case with the materials in practice at our disposal, that it shall be no larger than the quantity of water available under the particular circumstances of the case shall thoroughly flash. Pipes of from three to six inches internal diameter, are amply sufficient for service drains, as one of six inches, with a fall at the rate of one in forty will, according to the ordinary formula, discharge, in half-a-minute, as much water as is due to a family of six individuals for a whole day, even on the liberal scale of five cubic feet per head.



a, Carriage Pavement; *b*, Channel Stone; *c*, Curb; *d*, Foot-Paving; *e*, Coal-Vault Shoot; *f*, Gas-Pipe; *g*, Water-Pipe; *h*, Sewer; *i*, Syphon Trap; *j*, Tube; *k*, Coal-Vault.

The drains of streets should, in my opinion, be double in all the streets above twelve yards wide; and in such cases I would construct them in the bottom of the side trenches for the water and gas pipes, as shown in the annexed engraving. By this arrangement are insured diminished capacity and direct communication, and the other advantages connected with the paving, elsewhere insisted on. The first cost of the double sewer is nearly balanced by the saving in the service drains; and in regard to the paving, it is impossible sufficiently to estimate the saving, if, in connection with these side trenches, the more durable manner of paving recommended be introduced. The sewer proposed to be used in this case consists of a semi-oval underpart, formed of stone ware, or some of the other materials which will afterwards be noticed, and a semicircular cover, as shown in the above engraving. The joinings I propose to form with the pitch of coal-tar, in rather a soft state; and when any part of the drain requires to be inspected, a hot iron or a little blazing straw will soften the cement so much as to admit of the cover being removed. Service drains are connected with the side drains by means of sockets formed in the latter.

The report proceeds to denote the different forms of sewers and their connections, which are something similar to those described in the *Journal* for March last, p. 77. It then gives the area of land drained into each of the main sewers.

From observations made while Beacon's Gutter sewer (6ft. by 4 ft. 6 in.) was opened at its outlet, for the admission of the new sewer in Great Howard-street, it was found that the water was only 20½ inches deep, after twelve hours of heavy rain.

The extent of drainage into that sewer is 983·3 acres; of which about one-fourth is built, and three-fourths unbuilt. Now, by the formula most approved, the diameter of sewer necessary for this drainage, with the given fall, would be ten feet, while the actual diameter of the sewer at its largest part is only 4 ft. 6 in., and the depth of water in it was somewhat less than a third of its longest axis, even after the continuance of heavy rain for twelve hours, when it may be reasonably supposed the whole earth would be saturated, and every drop of rain would flow into the sewer. All calculations for the capacity of sewers proceed on the assumption, that it is necessary to provide for the contingency of a rain flood, estimated at the enormous fall of five-eighths of an inch in half-an-hour. Now, that such a flood may occur is possible; but it would be easy to show, that if it did occur, it could not get into the sewer, and therefore there is no necessity to make provision for it; and it requires merely a glance at the streets of a town built on sloping ground like Liverpool, during even a moderately heavy rain, to be convinced, that a great part of the rain drains directly by the surface of the street into the river, and never enters the sewers. As all formulæ then, are founded on imperfect experiments, and give results so far above what experience shows to be necessary, they are obviously uncertain guides, and it is better to trust to the observations of what actually takes place. This, in fact, is experimenting on the largest and most proper scale.

The Report afterwards makes some observations on surface drainage and paving. If the streets of a town be unevenly paved, putrid exhalations will constantly arise. A smooth, non-absorbent, hard surface, without hollows or joints, is what health demands. Macadamised streets are the worst: the absorbent material soaks up the liquid filth, which, putrifying, sends its noxious exhalations into the atmosphere; and the road wears fast under great traffic—in wet weather it is covered with mud, and in dry weather the air is loaded with dust. It is the most expensive to keep in repair, and costs four times as much to cleanse as a paved road. Boulder paving is the next lowest in the scale of roads. Streets formed with stones dressed in regular courses are the best: where the road is steep the courses should be of stone 3 inches wide, with joints 1½ inch wide, filled up with cementitious substance, impervious to water. In streets less steep, the stones may be increased and the joints decreased. On a level, the stones may be increased to 5 inches in width.

To insure stability, the courses should be made nearly wedge-form. They should be in contact at their base and for about one-third of their height, and the width of the joint should be obtained by diminishing the width of the upper surface of the courses. The joints should be rammed hard with macadam or clean shingle, and then filled with a coarse kind of asphalte, composed of the pitch of gas-tar and small gravel. The joint should not be filled quite flush with the surface of the stones, but left slightly hollow, as in the figure (*a*). If the expense of the asphalte be objected to, lime grout may be used to fill the joints. The lime should, for this purpose, be such as will set under water.

For the foot-pavement, Caithness stone, 1½ inch thick, with sawn joints, is recommended in preference to Yorkshire stone 3 inches thick.

A good foundation to a road is no less essential than a good surface; hardness in the latter cannot be insured without firmness in the former. Softness or elasticity in a foundation will permit the surface to yield under the wheels of a carriage. The rise and fall forms a new obstacle to be overcome, and causes an increased amount of friction to be opposed to the moving power. Besides, by the sinking of the paving material, the soft earth is forced up between the stones, and covers the road with mud. It is worth while, then, to be at a little extra cost in the preparation of the